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ORIGINAL LECTURES.

CARTWRIGHT LECTURES.

ON THE EXPERIMENTAL METHOD IN MEDICAL SCIENCE.

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LECTURE II.

BUFFON AND BONNET, IN THE EIGHTEENTH CENTURY.

MR. PRESIDENT AND GENTLEMEN: I shall ask your attention in this evening's lecture to two remarkable phases of physiological doctrine which once held a conspicuous place in the world of science, namely, Buffon's theory of Organic Molecules, and Bonnet's theory of the Inclusion of Germs. Notwithstanding that both are now so obsolete that, perhaps, the majority of my hearers will hardly recall their meaning, they were formerly considered of sufficient importance to be test questions in the controversies of the period, and they are fair examples of the fluctuating estimate to which such subjects are liable at different times.

It occasionally happens that, in searching the annals of medicine, we find allusions to systems and theories which have so far gone out of vogue that we know them only by name. They are no longer part of our scientific doctrines; and they have so little present value or significance that we seldom spend the time even to weigh their merits or defects—and yet these systems once had their day. They were prominent topics of discussion and interest; and they divided medical opinion between the views of antagonistic parties, or carried with them the scientific world in a temporary enthusiasm of acceptance and admiration. They were often originated by men of learning and ability, and owed much of their success to the superior talent of their authors. They were usually ambitious in design and seductive to the imagination. They claimed to open a wider field of knowledge, and to unlock the secrets of nature by some new formula that should dispense with the older and slower methods of plodding investigation. So long as they held out this prospect, they stimulated the hopes and attracted the interest of all. But after a time it appeared that the anticipations which they excited were not fulfilled, and that their promised results failed of realization. Then the theories themselves began to lose in importance; and, once started on the downward road, they receded more and more rapidly from view, until they passed out of sight, and out of mind. They vanished into the limbo of departed spirits; and if they ever show themselves at the present day, it is only to gratify a momentary curiosity as the relics of thought and opinion in former times. Like unsubstantial shadows, they flit across the pages of a forgotten literature, and the glimpses that we catch of them, here and there, are the only traces of their existence in the history of the past.

One of these phantoms, alive and flourishing a hundred years ago, was Buffon's theory of *Organic Molecules.*

Buffon was a remarkable instance of a man of splendid but temporary reputation. His ability commanded the attention of his contemporaries, and enlisted their interest and co-operation for natural history, as no modern writer had done before. His immediate successors were fond of comparing him with Aristotle and Pliny; and if we take their estimate of his reputation, there have been few men, either in science or literature, who ever acquired so great a popular renown. He was a member of the French Academy, of the Academy of Sciences, of the Royal Societies of London and Edinburgh, and of the Academies of Berlin and Bologna. He was created a Count by Louis XV., and was for forty years superintendent of the Garden of Plants (then the "Garden of the King"), which he more than doubled in extent, and where his statue was erected while he was yet alive. His great work on natural history was received as an authority from the date of its publication; most of the European potentates sent him specimens for his museum, or complimentary messages; strangers from abroad thought it a privilege to see him; and among the visitors at his country house were Thomas Jefferson and the Prince of Prussia.

Notwithstanding this extraordinary success, Buffon's qualities could not secure a lasting reputation. He excited the enthusiasm of his readers by his eloquence and originality, but he did not give them much real instruction. This deficiency was felt, even by his friends. Viege d' Azyr, who pronounced his eulogy in the French Academy, and who calls him "one of the lights of the age, and an ornament to his country," says that he often "compels admiration, without convincing the judgment." According to Moreau, he "supplemented by the brilliancy of his imagination the imperfections of his knowledge," and showed himself "more remarkable for boldness of conception than for precision of ideas." There can be no doubt that he did much, in his own time, to popularize natural history among the educated and influential classes, but it was rather as an intellectual entertainment than a scientific pursuit; and his works are now nearly destitute of value for any purposes of reference or exact information.

It appears that Buffon's style as a writer was one of his principal charms for the readers of that day, and formed a large element in his success. It had a sort of poetic splendor, that would be thought quite inadmissible at present in a scientific work, and sometimes will hardly bear translation without a close approach to puerility; but at the period when he wrote, and in his hands, it magnified his subject, and made an impression upon his readers. He often adopts this lofty tone at the opening of a chapter, and nearly always employs it for descriptive passages. He introduces the horse as "this proud and impetuous animal, the noblest conquest ever made by man, his companion in the fatigues of war and in the glory of combats." "The elephant," he says, "makes the earth tremble beneath his footstep, uproots trees from the soil, and with one thrust of his body will batter down a wall." Buffon took great pains to cultivate and perfect this style. They say he would often repeat aloud certain passages, or get his friends to do so, with himself for an audience, to enjoy the sound of his swelling sentences, and, perhaps, make some improvement in their construction. One of the extracts which he was especially fond of hearing in this way, was his delineation of the feelings of the first man, when just awakened to consciousness and vitality. The newly created being is supposed to be telling his own story, as follows:[1]

[1] Œuvres Complètes de Buffon. Second Edition. Paris, 1819. Tome x., p. 358.

"I well remember that instant of mingled joy and anxiety when I felt for the first time my extraordinary existence. I knew not what I was, where I was, nor whence I came. I opened my eyes. What an overflow of sensation! The daylight, the celestial vault, the green earth, the crystal transparency of the waters, all attracted me, excited me, and gave me an inexpressible sense of enjoyment." And so on for seven or eight consecutive pages.

Another favorite passage was his picture of an Arabian desert, introduced *apropos* of the natural history of the camel.[1]

"Imagine," he says, "a region without verdure or moisture, a blazing sun, the sky always arid, plains of sand and mountains still more parched, where the eye searches in vain for any trace of a living creature; a land lifeless, and, as it were, stripped by the winds; where nothing is to be seen but dead bones, scattered pebbles, and rocks standing or overturned; a barren desert, where the traveller never finds a shady resting-place, where he has no companionship, nor anything to remind him of life in nature; absolute solitude, a thousand times more frightful than that of the forest, since even trees are living beings for the man who is there alone. In these void and measureless regions, more isolated, denuded, and desolate than any other, the empty space seems to enclose him like a tomb. The light of day, more gloomy than the shades of night, returns only to disclose his impotence and destitution. It shows him the horror of his situation by enlarging before his eyes the boundaries of vacancy, and shuts him out from the habitable world by stretching around him an abyss of immensity which he could never hope to cross."

The satisfaction which Buffon felt in these descriptions does not seem to have been in the least diminished by the fact that he had never seen an Arabian desert, and that his story of the first man was entirely imaginary.

Buffon's *Natural History* was a work of very extensive design. It included an explanation of the structure and physical geography of the globe, the nature of man and animals, a general view of animals, vegetables, and minerals, the functions of nutrition and reproduction, an account of the varieties of the human race, and the different species of birds and quadrupeds, with a description of specimens in the Royal Museum.

He had a theory of the planetary system, which was one of his favorite doctrines, and which occupies the greater part of his first volume. He believed that the planets were originally a part of the solar sphere, from which they had been struck off, at some time, by the shock of a comet in collision with the sun. The materials of different densities being propelled under this impulse with different velocities, he supposed would naturally collect into spherical masses at corresponding distances from their starting-point, and so give rise to a series of planets moving in successive orbits. Their axial rotation was accounted for by the obliquity with which the comet struck the sun, and the consequent twirling motion imparted to its detached portions.

Wild as this hypothesis sounds, when stated by itself, it was advocated by its author with much force and ingenuity. Even mathematics came in for a share in its support, and it was fortified with arguments from the doctrine of probabilities. As all the planets move round the sun from west to east, and nearly in the same plane, they must all have been set in motion, according to Buffon, at the same time, and by a single impulse. Otherwise, the chances would be as 64 to 1 that the six planets then known would not all move in the same direction; and as 7,962,624[1] to 1 that all their orbits would not be within 7½ degrees of the same plane. He extended the calculation still farther, to the size, distance, and density of the different planets, and to the relation in each between its density and the rapidity of its orbital motion. The figures did not all come out exactly right, but in the case of Jupiter and Saturn the theoretical and actual proportions were very nearly alike, and the author observes with much satisfaction, that "we can rarely obtain from pure conjecture so close an approximation to reality as this."

But the most remarkable production of Buffon's genius was his system of organic molecules, with its associated theories of nutrition and reproduction. According to this view, the bodies of all living creatures, animal and vegetable, were composed of minute particles differing in kind and configuration, but all possessing the general character of vital endowment. These were the organic molecules. As we call a compound group of hydrogen and oxygen atoms a molecule of water, or one of carbon, hydrogen, and oxygen, a starch or glucose molecule, so Buffon seems to have imagined a still more complex molecule having the properties of organic life. These molecules, when associated in the form of an organized body, gave to each part its specific character, and thus provided for the physiological activity of the whole. But they were themselves indestructible, and contained in their own substance the essential principle of vitality. When the animal or plant died and was decomposed, its organic molecules were at once ready to assume new combinations. When one animal fed upon another, or an animal upon a plant, the organic molecules of the creature used as food passed into the tissues of that which devoured it, and when an animal in decomposing, enriched the soil and fed vegetation, its organic molecules were absorbed into the structure of the growing plant. Thus these molecules represented the living elements of the material world. Their quantity was never either increased or diminished. They only passed from one form of combination to another, always transferring their activity from the old organisms to the new. Consequently, there was no such thing as destruction of life in nature, but only continual changes in its distribution. The organic molecules were an exhaustless reservoir of vitality, from which all living beings took their origin, and into which they all again returned.

But nutrition and generation require something more than the mere existence of organic molecules. For even supposing these vitalized particles to be present in full abundance and variety, how could they be arranged in proper order, within the plant or animal, so as form its different organs or enable it to reproduce its like?

This want was supplied by Buffon's celebrated idea of *interior moulds*. It is not easy to say exactly what he meant by this expression, except that it was something capable of arresting the molecules and placing them in a certain combination. "In the same way," he says, "that we have moulds for giving to the outside of things any form we choose, suppose that nature can produce moulds which will determine, not only the external configuration, but also the internal structure of bodies formed in them."

Most readers will perhaps say that this definition of interior moulds is only an analogy, and does not include any intelligible idea of their construction. Buffon himself seems partly conscious of this, and makes a rather labored and metaphysical defence of the term, to show that there is nothing in it contradictory. However that

[1] Œuvres Complètes, Tome xv., p. 336.

[1] Uranus was only discovered in 1781, thirty-two years after the appearance of Buffon's first volume. If the author had included Uranus and Neptune in his calculation, he might have made the chances in favor of his theory as 4,586,471,424 to 1.

may be, the "interior mould" is something which belongs to the entire body and also to each of its separate parts. The whole body is a mould which determines the position and arrangement of its different organs. Each organ, and each part of an organ, when supplied with organic molecules from the food, selects those which are like its own and rejects the rest; while it arranges the incoming molecules in proper order, and incorporates them with its substance, still preserving its original form and texture. In this way the body is nourished and grows, not by addition to its exterior, but by the intimate penetration and selection of appropriate molecules throughout its mass; and all by means of its "interior moulds."

The mysterious phenomena of reproduction were fully explained by the aid of organic molecules and interior moulds. This explanation was even the simplest in cases which we usually consider the most wonderful; as where plants, worms, or polypes reproduce and multiply without sexual generation, by the processes of division or budding. Suppose, in a polype, the organic molecules to be arranged throughout in the form of minute polypes, just as a cube of salt is made up of an indefinite number of smaller cubes, then any part of such an organism, when separated from the rest, will reproduce an entire polype, because it already contains the whole of it in miniature. In an animal or plant which multiplies by budding, the organic molecules absorbed with the food are distributed in due proportion until every part has received its full supply. After that, the surplus molecules are sent back from the different organs and collected in some particular locality, where they form a composite mass, representing the materials of the whole body. Such a mass is a bud; and, when separated from its stock and placed where it can absorb nourishment for itself, it of course grows into an organism like its parent, because it is composed of the same kind of molecules, arranged in the same order, and in the same proportions.

In sexual generation, like that of man and the higher animals, the process is more complicated. Here, again, every part of the body absorbs the organic molecules appropriate to itself, and moulds them into the substance of its own texture. But the excess of material, not required for the nourishment of the organs and returned from them as superfluous, appears in the form of a liquid, and is deposited in a special reservoir. This liquid, made up of contributions from every part of the body, is the seminal fluid. It is a mingled organic extract of the entire frame, and thus contains all the materials necessary for reproduction. Its place of deposit is in the testicles and vesiculæ seminales.

The extreme activity of this seminal fluid, the concentrated essence of the animal organism, is proved by the formation of the spermatic animalcules. According to Buffon, these are not original or essential ingredients of the seminal fluid, but a product of its restless vitality, an incidental form in which its generative power is exhibited.[1] They represent the first assemblage of its organic molecules, as combined into moving particles large enough to be visible by the microscope.

But the female also produces a seminal fluid in the same manner and with the same general qualities as the male. Both fluids, male and female, contain all the elements common to either sex. The only difference between them is that each fluid being a special extract of the body from which it comes, that of the male contains molecules representing the male organs, that of the female those representing the female organs. Buffon entirely rejects the doctrine that viviparous females produce by means of eggs. For him, the ovaries are testicles; the Graafian follicles are reservoirs of nutritious lymph; and the corpora lutea are glandular structures, which produce the female seminal fluid by a process of filtration.[1]

Thus Buffon's theory accounts for reproduction in a very simple manner. Generation is only, in a higher degree, the act of nutrition, operating with the surplus of nutritive material. A mixture of the male and female seminal fluids contains all the organic elements of both sexes, and, therefore, represents the species. In the cavity of the uterus it finds its appropriate nidus and nourishment, and grows into a perfect organism like that from which it came. The theory was supported by abundance of proof; for it explained to a charm most of the important phenomena of generation. 1. During infancy and childhood there is no power of reproduction, because all the organic molecules are taken up by the growing tissues; it is only after puberty, when the body has acquired its full growth, that there is a surplus of organic material, to be filtered through the testicles in the form of seminal fluid. 2. The young child or animal usually resembles both its parents, because its substance is derived from both the male and female seminal fluids. 3. Creatures of small size, like insects, mice, and guinea-pigs, have a numerous progeny, because the demands of nutrition are easily satisfied, and there is a large superfluity left for reproduction; but those of greater bulk, such as man, the horse, the elephant, produce only a single young one at a time, and at long intervals, since the nourishment of their own bodies takes up most of the organic molecules of the food. All these facts were plainly in accordance with the new system.

Indeed, the only difficulty about Buffon's theory (if it is not a contradiction to say so) was that it made the matter too easy. If the seminal fluid of the male contains all the organic molecules necessary to make an embryo, why does it not make one? Why should not the male be self-producing? And why should not the female, on the other hand, have a progeny of little females from her own seminal fluid without the necessity of impregnation by the male? According to the theory of organic molecules, every adult animal produces a fluid capable of generating young. But, in point of fact, the female must also receive the fluid of the male; and, without the concourse of the other, each sex remains barren.

This is the great difficulty in Buffon's way; and, to do him justice, he does not try to conceal or evade it, but struggles with it manfully for sixteen octavo pages. The reader almost feels a sympathy for him when his system of reproduction, after running so smoothly everywhere else, is arrested by so bulky an obstacle as the fact of sexuality. His explanation is full of ingenious hypotheses, but it is very complicated, and leaves in the chain of evidence so many gaps and contradictory possibilities that it is hardly worth while to remember it. Nevertheless, the author thinks too well of the system to have his faith in it shaken by one or two imperfections. The main part of it, he says, he has "proved by so many facts and arguments, that to doubt it is hardly possible." He does not doubt it himself, and he avows that he "entertains no uncertainty whatever as to the basis of the theory, after careful scrutiny of its principles and the most rigorous estimate of its consequences and details."

Buffon's system has its widest extension and most splendid success in the dim regions of equivocal and spontaneous generation. His organic molecules are the genuine repositories of life; the interior moulds of particular organisms being only the means of arranging them in special forms. And when, by the death of an animal, and the decomposition of its body, its organic

[1] Œuvres Complètes, Tome ix., p. 335.

[1] Ibid., p. 345.

moleculès are released from the constraint of their interior moulds, they are set at liberty, living and active as ever, to be disseminated in any direction where chance may carry them. In this condition they are the universal seed of nature; without any character of specific organization, but containing all the prolific force of the bodies from which they came. After a time they are absorbed by some other mould, animal or vegetable, into which they transport their own indestructible qualities of nutrition and life. But during the interim, after leaving the old organism and before their incorporation into the new, they form by their reunion a multitude of living structures which do not belong to any of the ordinary or recognized species in nature. Such are the moving corpuscles of the seminal fluid, the microscopic fungi and infusorial animalcules of organic solutions, and, in their larger varieties, the eels of starch-paste and vinegar, and even earth-worms, mushrooms, and entozoa. All these creatures come by spontaneous generation. They are the connecting link in nature between the simple organic molecule, on the one hand, and the complete animal or plant on the other. This exuberant force of production is always in activity. Notwithstanding the death of any number of existing creatures, the total quantity of life on the globe cannot be diminished even for a moment; for it continues uninterrupted in the organic molecules, which pass and repass, from the animal to the vegetable, and from the vegetable to the animal, or produce in the interval an endless variety of spontaneous generations.

No doubt Buffon was in love with his system, and admired it for its own sake. Still, he had a fair share of what Condorcet calls the "sentiment of his own superiority," and ends one of his chapters in a strain that sounds, at this day, rather exalted. "I will add nothing more," he says, "to these reflections. To be accepted, or even to be comprehended, they require a profound acquaintance with nature, and complete freedom from prejudice; so that for the majority of my readers, a longer explanation would still be insufficient, and for those who can understand me, it would be superfluous."

When Buffon's work appeared, it attracted at once the curiosity and applause of the public. The reviewers praised it as "an honor to both the age and the nation."[1] Hume said that its author "gave to things, no human eye had seen, a probability almost equivalent to proof;" and Needham declared that his investigations on the seminal fluid would be a subject of admiration "for centuries to come." All his readers, however, were not of that opinion. There were some who did not accept the doctrines of the book, and who feared that its popularity might have a bad effect. Buffon's high position, his magnificent style, and his confident tone of assertion, alarmed his adversaries, and made them dread his influence on the public mind. His theory, they said, was "filled with dazzling paradoxes, as disastrous to the principles of science as the mines of a skilful engineer to the solid ramparts of a fort." This difference led to a warm discussion. The Abbé de Lignac wrote an anonymous treatise against Buffon's work, under the title of *Letters to an American*,[2] where he criticised the system of moulds and molecules, and said that, instead of describing natural objects, it contained only "the philosophical dreams of M. de Buffon." Buffon's friends, on the other hand, denounced the "American Letters" as a lampoon. They accused de Lignac of having written it out of envy, and to punish Buffon for not showing more respect to his friend Reaumur. Deslandes called the book an imposture, because it was entitled *Letters to an American*, when it had not been written, in the form of letters, to anybody; and because the title-page bore the imprint of Hambourg, though it was really printed at Paris. "That makes no difference," replied de Lignac, "the only title-page that can be called an imposture, is one that promises something the book does not contain; like a natural history, for instance, made up of disjointed and extravagant systems, without anything natural about them."

Buffon's critics were especially dissatisfied with the organic molecules of the seminal fluid, and their wonderful capacity for uniting into an embryo. "If these molecules," they said, "are indifferently the elements of a plant, a man, a dog, or a polype, how can they combine to make either? We might as well go back to Epicurus and his atoms." One of them was asked, by an admirer of Buffon, which he would rather be, the author of the *Histoire Naturelle* or that of the *Lettres Américaines*. He replied that he would rather be an organic molecule, because then he would have more intelligence than either of them. Buffon held his own against these attacks without making any direct reply. His literary talent alone was almost enough to secure him the victory; and if his opponents denied the truth of his theory, they generally had nothing better to offer in its place.

The only real investigations at that time which seriously threatened Buffon's system were those of Haller on fecundation and development. They first appeared in 1753, four years after Buffon's first publication. Haller had examined the ovaries of sheep both before and after impregnation, and observed the condition of the Graafian follicles and corpora lutea. Since corpora lutea, according to Buffon, were glands for producing a seminal fluid, they must, of course, be formed before impregnation and could only open, to discharge their fluid, at the time of conception. Haller declared that there were no corpora lutea in the ovaries during the period of heat, nor immediately after conception; at which time there was nothing to be seen but a simple orifice leading into a ruptured follicle. The glandular structure of the corpus luteum was only formed some days afterward, and, therefore, could not have furnished anything essential to conception.[1] We now know the accuracy and value of these observations by Haller, and can hardly understand how those of Buffon should have been so preposterously incorrect. But at that time it was difficult to decide between them. Both were men of high scientific reputation, and the system of organic molecules was too promising and seductive to be surrendered at the first attack.

But, as time went on, it began to suffer from its own defects. After thirty or forty years' trial, its promises had not produced anything, its obscurities had not been cleared up, and the organic molecules were still as hypothetical as ever. One of Buffon's tenets for explaining reproduction by division, was that the simpler organisms, like plants and polypes, were composed of particles in the form of their own miniature, and he had argued at great length that this kind of structure, if we only thought so, was as simple as any other. Malesherbes[2] finally concluded that it would be of no use to think so, because the thing itself was impossible. "To be convinced of this," he says, "let any one try to draw a picture of this pretended polype entirely made up of similar polypes; or let him get a quantity of little wooden or ivory figures in the form of polypes, and then see whether he can put them together in any way so as to make an entire polype of the same shape. I defy him to do so."

[1] Journal des Savans. Paris, Octobre, 1749.
[2] Lettres à un Ameriquain, sur l'histoire naturelle, de M. de Buffon, à Hambourg, 1751.

[1] Histoire de l'Académie Royale des Sciences, Année 1753, p. 134.
[2] Observations sur l'histoire naturelle de Buffon, Paris, 1798.

Buffon's interior moulds had no better luck in the end. When it came down to genuine matter of fact, no amount of verbiage could make it plain what sort of things they were nor how they operated. Malesherbes declared that the very idea of a mould is that of something which acts by its surfaces; and that consequently the hypothesis of interior moulds is a mere metaphysical subtlety, without any real application. "To make use of interior moulds, therefore, for explaining the process of nutrition or development, is to solve one difficulty by another; it accounts for one thing by supposition of a second no more intelligible than the first. The truth is, it does not really give any explanation at all; it only answers, like the doctor in Molière, '*Opium facit dormire quia habet facultatem dormitivam.*'"

Another cause of decline for Buffon's system, towards the end of the last century, was the progress of opinion in regard to spontaneous generation. At that time there was much investigation on this subject, especially as to the infusorial animalcules. Needham and Spallanzani were the principal disputants in this field, and attracted to it for some years the attention of the scientific world; the result being at last, a general conviction that Spallanzani's experiments had destroyed the hypothesis of spontaneous generation, at least for the infusoria. The system, therefore, diminished in importance, because this mysterious department of reproduction, which it had explained so well, no longer needed explanation. It thus lost one of its strongest claims to support; and, without being actually disproved, it was discredited to an extent which must have seriously impaired its scientific status.

Finally, it received its death-blow in 1827 from De Baer's discovery of the ovule in the ovarian follicle of mammalians.[1] De Graaf had supposed, when he first described the ovarian follicles, one hundred and fifty years before, that they were really eggs, and this view was widely accepted. The ovaries of mammalians, like those of fishes and birds, were regarded as clusters of eggs, held together by intervening tissue. But it was not easy to demonstrate, in man and quadrupeds, the discharge of these supposed eggs from the ovary. No observer had ever succeeded in finding one of them separated from its ovarian connections; and the earliest ovum discoverable, in the uterus or Fallopian tube, was of course considerably smaller than the ripe follicle in the ovary. This threw a doubt over the doctrine of De Graaf, and left room for Buffon's assertion that there were no such things as eggs in the mammalian female; that her so-called ovaries were testicles, as much so as those of the male, and that she produced a seminal fluid, composed of organic molecules, similar to his.

This view was so interwoven with Buffon's whole system, that one could hardly exist without the other, and when the microscopic ovum and its mode of exit from the ovary were at last fully brought to light, so that any one could see them when he chose, the discussion necessarily came to an end. The female testicles and seminal fluid, with their glandular corpora lutea, no longer had a place in court; everything connected with them shared in the collapse, and the entire company of organic molecules, reproductive extracts, composite organisms, and interior moulds disappeared together, like actors at the end of a play.

Another set of ideas, once in high repute, was Bonnet's theory of the *Inclusion of Germs*.

This system was somewhat subsequent to Buffon's, though its author was for many years his contemporary. Bonnet was of a wealthy and influential family in Geneva, where he passed the whole of his life, engaged in the study of natural history and kindred subjects. He became widely known for his attainments in this direction, and was connected with nearly all the learned societies of Europe. He was the friend and correspondent of Reaumur, Haller, Spallanzani, Trembley, and De Saussure. Spallanzani communicated to him in manuscript, for perusal and advice, his own researches on the generation of infusoria; and when the volume appeared, he printed, as part of it, Bonnet's letters in reply.[1]

The personal character of Bonnet had traits of superior excellence, and secured him a large share of attachment and esteem. His disinterestedness and modesty, in regard to his own opinions, were well known, and were often the subject of marked eulogy. He attributed the principal credit for his discoveries in entomology to Reaumur, because he had been inspired by the example and encouragement of this master, under whom he felt "proud to enroll himself as a pupil." Spallanzani calls him the "Philosopher of Geneva," and the "sublime author of the contemplation of nature." His first publications appeared in 1745, and were followed by others at various dates until 1770, after which they were collected in a series of eighteen volumes.[2]

Bonnet presented the curious spectacle of a man under the varying influence of two opposite tendencies; at one time an exact and painstaking observer of nature, at another an irresponsible wanderer in the vaguest of speculations. His earliest scientific work was an experimental investigation on the reproduction of aphides, by which he first demonstrated with certainty the non-sexual viviparous propagation of these insects.[3] It is hardly possible to imagine a more thorough, direct, and conscientious method than that adopted by Bonnet in these observations. He took the young aphis, just expelled from the body of its parent, placed it upon a sprig of fresh leaves, stuck in a pot of earth and covered with a bell-glass, and then watched his solitary prisoner for twenty-one days, until it had grown to maturity and had produced, without fecundation, ninety-five young ones. During this time he kept exact records of the insect's moulting and also of the new births, most of which took place under his own eye. He repeated the trial with aphides of different species, and with those which had themselves been produced in captivity; and he succeeded in raising them in this way, without fecundation, for eight successive generations. For these observations he received the honor of an election as Correspondent of the French Academy of Sciences. He also made valuable contributions to natural history on the multiplication of aquatic worms by division, on the growth of plants, on the function of leaves, and on the habits of insects.

On the other hand, he constructed, out of his own meditations, a graduated system of things visible and invisible which he called the *Scale of Being*, and which formed the subject of one of his later works, published under the title of *Contemplation de la Nature*. It embraced the structure of the entire universe, from the simple elements of inorganic matter to the "Celestial Hierarchies" of other worlds. According to this system, there are no gaps or sudden variations in nature, but only a continuous series of combinations and modifications in ascending grade. Everything is immediately connected with that which precedes and with that which is to follow it. At the beginning of the volume there is inserted, for the reader's convenience, a printed table exhibiting the regular succession of natural

[1] K. E. von Baer. De ovi mammalium et hominis genesi, Lipsiae, 1827.

[1] Spallanzani, Opuscoli di fisica animale et vegetabile.

[2] Ch. Bonnet, Œuvres d'Histoire naturelle et de Philosophie. Neuchatel, 1779.

[3] Œuvres de Bonnet. Tome i., p. 19.

objects from gaseous substances up to man. The purely inorganic elements in various grades of combination lead to the composite earths, sulphurs, and metallic bodies; thence through the vitriols, or metallic salts, to ordinary saline substances in crystalline form, and thence to compound rocks of heterogeneous structure, like granite and sandstones. Immediately above these are rocks like slate, talc, and asbestos, which exhibit a laminated or fibrous texture, and which may therefore be considered as "the connecting link between inorganic and organic solid bodies." This point is especially important in Bonnet's scheme, for it marks the passage from dead matter to living organisms. It is not altogether what its author could wish, but he makes the best of it by quietly putting its burden of proof on posterity. "It must be acknowledged," he says, "that the transition here is not quite so perfect as in many other cases. It looks as though nature, in this instance, had made a leap, but the gap will undoubtedly be filled up as our knowledge increases in precision and extent."

Once fairly started among organized structures, the scale rises easily through corals, lichens, and mushrooms to plants in general; through the sensitive plant to animals, from insects to snails, serpents, eels, and fish; through flying fish to birds, from birds to bats and quadrupeds, and so on to monkeys, apes, and man. But this extended gradation is not enough for Bonnet. He has also the "Celestial Hierarchies" and the "Gradation of the Worlds," some of which, he is persuaded, are as superior in physical and moral organization to our earth as man is above the ape.

The frontispiece to Bonnet's work is an engraved portrait of the author, taken, as he tells us, when he was "plunged in deep meditation on the restitution and perfectibility of living creatures." It renders, he thinks, his "reflective expression" with much success. One cannot help thinking it fortunate that the artist saw him just when he happened to be meditating on so abstruse a subject.

Bonnet's great theory of reproduction, or the "Inclusion of Germs," appears mainly in his work, entitled, *Considérations sur les Corps Organisés*, with some additional amplifications in the *Contemplation de la Nature*. His thoughts were turned in this direction partly by Trembley's recent discovery of the multiplication of fresh-water polypes by division, partly by his own observations on aquatic worms, and on the reproduction of aphides. He was further strengthened in his opinion by Haller's observations, which appeared in 1758, on the formation of the chick in the fowl's egg.

There were, about that time, three different views entertained as to the nature of sexual generation.

I. The spermatic animalcules of Leeuwenhoek, one or more in number, were arrested in the uterus, and there became developed into embryos. According to this view, the male parent furnished the living germ, the female only providing for its care and nourishment.

II. The seminal fluids of both sexes, when mingled together, produced the embryo, *de novo*, by a mutual arrangement of their organic particles. This was the doctrine of Buffon, and attributed an equal and similar action to both parents.

III. The germ *pre-existed* in the ovary and egg of the female, and was only stimulated to further development by the seminal fluid of the male.

Bonnet accepted the third of these doctrines. He believed in pre-existence of germs, but he gave to the idea a much wider extent and significance. The germ as he understood it was not only a structure capable of being developed into a living creature; it actually contained, within itself and ready formed, all the parts of the future organism. These parts were exceedingly minute, delicate, and transparent, but they nevertheless existed, though as yet imperceptible to the senses. It was only their small size, their softness and transparency, which prevented their being seen. After impregnation they grew larger, more consistent, and more opaque, and then first became recognizable as the organs of the embryo. These organs accordingly were not things of new formation. They only showed plainly, at a certain period of development, what had previously existed in invisible form. The author extended this doctrine without hesitation to its farthest limits. For him every seed contains a plant in miniature. In an animal germ, every organ and tissue, without exception, is already present—the heart, the vessels, the liver, the kidneys, the intestine, the nerves, and the brain. Consequently, there is no such thing as generation, if by that we understand the production of a new being, or even of a new organ. What appears to be such is only the development of something that was in existence beforehand.

These views were adopted by some and rejected by others, the physiological theorizers of the day dividing into two parties, known as the *Epigenesists* and the *Evolutionists*. The epigenesists were those who believed, like Buffon, that the embryo was a new formation, produced by the union of molecules and the successive organization of different parts. The evolutionists,[1] on the other hand, maintained with Bonnet the pre-existence of germs, and insisted that nothing in the embryo was newly formed, but only enlarged and solidified by development.

These notions were sustained in some degree by the discoveries of Haller in the incubation of the fowl's egg. Haller had studied the development of the chick day by day, and had noted the appearance of its different organs and envelopes. His masterly observations on the yolk-sac and its connection with the embryo, were held by the evolutionists to be decisive as to the pre-existence of the germ and its essential parts. "The membrane enclosing the yolk," says Bonnet, "is continuous with the intestine of the chick. The arteries and veins of the yolk are branches of the mesenteric vessels of the fœtus. The blood circulating in the yolk vessels receives its impulse from the fœtal heart. The yolk is therefore essentially a part of the chick. But the yolk exists in the egg before impregnation; consequently, the chick also exists in the egg before impregnation. . . . . This proves incontestably that the embryo is at first a part of the parent fowl, and that its existence is anterior to the time of conception."

Bonnet's assumption of the pre-existence of the embryo, as well as of the germ, was not without a certain plausibility. The organs of the embryo, when first formed, are in fact not only small, but also colorless and transparent; so that they are seen with considerable difficulty, and may even be overlooked for some time after they are really present. Bonnet says, this is the mistake made by the epigenesists. "They estimate the time when the parts of an animal are first created by that at which they first become visible; and whatever cannot be seen, they assume has no existence." But the organs in an imperfect embryo might very easily have been there, though not sufficiently distinct to meet the eye. "Do you wish," he says, "a short and ready demonstration of this? When the lung of an embryo chick is first perceptible to the senses, it already measures ten one-hundredths of an inch. It is evident that with a size of only four one-hundredths it would have been visible, were it not for the extreme transparency of its tissue."

And so on, of all the other parts. Bonnet has more

[1] It is unnecessary to say that this term was used, during the last century, in a sense entirely different from that which attaches to it now.

faith than the epigenesists. He believes that the organs of an embryo were there before he could see them, and that they even existed from the beginning, though invisible, in the unimpregnated egg.

But if the germ contains all the organs of the chick ready formed, only small and undeveloped, it contains ovaries as well as the rest; and these ovaries contain the germs of the next generation. It is plain the series cannot end here. The germs of the second generation are chicks in miniature, as much as those of the first. Consequently, they contain their own ovaries and germs, which in turn contain others still; and so on without any definable limit, one set of germs within the other in geometrical progression, each germ containing not only its own embryo, but also all the embryos that can possibly be produced from it in unnumbered generations hereafter.

Bonnet does not shrink from the magnitude of this spectacle. He rather glories in it, and enjoys the contemplation of its unlimited perspective. "For my part," he says, "I like to extend as far as possible the limits of creation. There is a pleasure in the thought of this magnificent series of organized beings, enclosed, like so many little worlds, one within the other. I see them receding by degrees, diminishing in regular order, and losing themselves at last in an impenetrable night. It gives me a sense of satisfaction, when I look at an acorn, to see in it the germ of a majestic oak that centuries hence will shelter the birds and the beasts under its branches; or still more to behold in Emilia's womb the germ of a hero who thousands of years hereafter will found an empire, or perhaps of the philosopher who shall discover to mankind the cause of gravitation, the mystery of reproduction, or the mechanism of our existence."[1]

This prospect, however, for the germs of the future which Bonnet found so attractive, was not as satisfactory to some of his contemporaries. They even haggled with him about the past. If the age of the world, as generally admitted, was six thousand years, and if each generation of man is completed in thirty years' time, that would give two hundred successive generations since Adam and Eve, with about a thousand million human beings at present on the earth. All these inconceivable members must have been contained under the form of germs in the ovaries of Eve. The minuteness of the embryo of the two hundredth generation, when enclosed in the body of our first parent, was declared to be something incredible. Still more so, for plants which grow and fructify every year. Hartsoeker calculated that the relation in size between the first wheat-grain and that to be produced at the end of six thousand years would be expressed by the figure of unity, *followed by thirty thousand zeros.*

All this did not give Bonnet or his friends any discomfort. Such terrific estimates may serve to confound the imagination, but logic always finds a sure refuge in the infinite or indefinite divisibility of matter. Hartsoeker and his followers, according to Bonnet, put the senses and the imagination in place of the understanding. They want to see and touch, so to speak, what can only be comprehended by an effort of the mind. The hypothesis of the inclusion of germs is above the level of such calculations, and is, he concludes, a "splendid triumph of pure reason over the senses."

Bonnet's theory is by no means confined to the simple hypothesis of one germ included within another. It is an extensive and elaborate system, ramifying into all departments of natural history and physiology. The included germs not only exist before fecundation, but they are nourished, and grow in a way analogous to that of adult bodies. There can be no doubt of this, because the fowl's eggs certainly increase in volume before fecundation, and are to be seen in the ovary of many different sizes and grades. But the yolk is essentially a part of the young chick. Therefore, if the yolk grows before fecundation, the embryo does also. Beside, in the theory of inclusion, the contained germs diminish progressively one within the other, to an inconceivable minuteness. The outermost germ is, of course, the largest, and is nearly ready for impregnation. But when it has been fecundated, and has expanded into the body of an adult female, the next included set of germs have in turn come to be as large as the other was before, and so on in continuous order, the smaller germs gradually moving up in the scale of magnitude, to be ready for impregnation when their time arrives. The process of nutrition and growth must, therefore, be going on in these embryonic organisms, and has been going on in them ever since the creation.

But how is the nutrition of the germs provided for, and what are the materials used in their growth?

It is well known, says Bonnet, that the different parts of the body are not nourished directly by the blood. A more delicate and colorless fluid, called *lymph*, separated from the blood by appropriate organs, serves for the immediate supply of the tissues, and as these tissues vary in fineness and consistency, we can hardly doubt that the secretory organs prepare for them different kinds of lymph, more or less attenuated to correspond with the delicacy of structure in separate parts. Some tissues, as, for example, the brain and nerves, must require a lymph many times finer and more subtle than that employed for the grosser parts.

But the included germs are made up of tissues finer still. If we admit that there is no such thing as real generation, and that everything has been preformed from the beginning, then the germs which are to make their appearance a thousand years hence, have now actually within them, and incalculably minute, all the parts characteristic of the species. Imagine the heart, the brain, the nerves, and the stomach of such an embryo. Certainly, no lymph could penetrate these tissues, or be of the least use for their nutrition. What fluid is there in the body sufficiently attenuated and ethereal to insinuate its particles into a structure of such delicacy, and incorporate them with its substance? There is only one, and Bonnet secures it. This precious and double-distilled material, which is alone capable of nourishing an infecundated germ, is the *nervous fluid.*

The nervous fluid was not an invention of Bonnet's. It was generally acknowledged, in his time, as the immediate cause of nervous action. Like the electric fluid, it was intangible and invisible, and was known only by its effects. Its invisibility was due to its vapors or aerial constitution; and this enabled it to circulate in the delicate passages of the nervous tissue, which were themselves invisible, owing to their minuteness. The nervous fluid, or "animal spirits," so-called, after being elaborated in the brain, was disseminated through the nervous channels to the various organs of sense and motion. Its existence was proved by a very simple fact. If you tie a nerve going to a muscle, the muscle is paralyzed. How could a ligature cut off the nervous influence, except by arresting the passage of a fluid? There was, therefore, little or no doubt in regard to it.

The nervous fluid, in Bonnet's system is not entirely dissipated in the exercise of sensibility and muscular action. The unconsumed portion, after returning to the brain, was again sent out by similar channels, to be used in nutrition. It was carried by the maternal nerves to the ovaries, and there immediately distributed to the germs "of the first order," or those nearest maturity.[1] Elaborated anew and still further spiritualized by the organs of this delicate

[1] Œuvres de Bonnet, Tome v., p. 206.

[1] Œuvres de Bonnet, Tome x., p. 10.

structure, it supplied a finer nourishment to the germs of the second order; here it underwent a similar preparation for those of the third order, and so on, to the belated and unfortunate germ which "would not be ready for fecundation till the end of the world."

This staggering array of infinitesimals, in structure and function, only serves to increase the author's admiration and strengthen his confidence. "What an abyss of wonders," he says, "in the human brain. Even a single fibre of the brain is itself an abyss. What shall we say, then, if all these mysteries, existing in the adult body, are repeated and concentrated in so many organized atoms, enclosed one within the other, and growing constantly smaller, in indefinite progression?"

The act of impregnation, in reproduction by sexes, comes in for a very easy and natural explanation. The nervous fluid is the appropriate nourishment for an infecundated germ. When this germ becomes a fœtus, it will be supplied with lymph, and its organs will grow like those of the adult. But how can it make the transition from one state to the other, and how are the finely woven tissues of the germ-embryo to be made capable of admitting the grosser substance of the lymph? This is exactly the function of the seminal fluid. It is not only a source of nourishment for the mature germ, but it has a special stimulating influence on the embryonic heart. The pulsation of the heart in the germ-embryo is very faint, and only serves for the circulation of the nervous fluid. Consequently, the infecundated germ increases but slowly, and never gets beyond a certain stage of development. But when the seminal fluid comes in contact with it, and penetrates its substance, the heart's pulsation is increased in force. This causes an expansion of the circulatory channels, an increased volume of the circulating fluid, and, consequently, more rapid nourishment; and thus the limit is passed between the condition of the germ-embryo, whose tissues only admit nervous fluid, and that of the growing fœtus, that can thrive upon the lymph. Once begun, the process naturally continues until the body has attained its full development.

Still the theory of inclusion was not altogether free from difficulty. One of its most troublesome problems was that of intestinal worms and other parasites. Bonnet wrote a special treatise on the tænia, partly devoted to different theories of its origin. He rejects, of course, the idea of equivocal or spontaneous generation. But if, like everything else, it comes from pre-existing germs, are these germs introduced into the intestine from without, or did they exist beforehand in the embryonic human body itself? If so, the germ of the first tapeworm, and, in fact, of all which have existed since, must have been included in the body of the first man. There are objections to be met in either case, but the most singular are those connected with the second.

In this view the tænia has a contemporaneous origin with man; that is, it already existed in Adam and has passed so into his posterity. But according to Genesis, the animals were all created before Adam. Therefore, the tænia must have first lived in a foreign element; and the fact is, we never find it except in the intestines. Besides, how can we suppose such a pest to have been placed in the body of our innocent progenitor, living in the earthly paradise of the Garden of Eden? Valisnieri, who adopted the contemporaneous theory, had an odd answer to this objection. He assumed that before Adam's fall the intestinal worms did not do him any harm, but, on the contrary, were rather useful in "consuming the superfluous humors and gently stimulating the intestinal walls." Bonnet thinks this explanation a trifle far-fetched, and doubts whether Adam, in his state of innocence, had any superfluous humors or needed any tapeworm to excite his intestines. He prefers the idea of M. Leclerc, that the intestinal worms, which are now so troublesome, only existed at first in Adam under the form of eggs, and were not hatched until after his disobedience.[1]

But the line of descent for tapeworms is not yet quite clear. Granting that they existed in Adam, either as eggs, or as worms, how did they get into the body of Eve, through which they must have passed, for transmission to posterity? The most direct route from the intestine, according to Valisnieri, would be through the thoracic duct, which "mounts upward along the ribs," to that one which was taken for the body of Eve. Bonnet is inclined to adopt a simpler and less miraculous method of transfer, by supposing that the eggs of the tænia are so small that they can pass through the bloodvessels, and thus reach the vesiculæ seminales. "In this way," he says, with a puzzling kind of *naïveté*, "we can explain the whole thing without much difficulty, and *without having recourse to divine power*."

However, the author regards the introduction of parasitic germs from without as on the whole the more probable theory of their origin, especially as the weight of evidence was already in that direction.

The system of Bonnet has applications to many different points of general physiology, such as the resemblance of the young animal to both parents, the production of hybrids and monstrosities, and in particular to non-sexual generation, or multiplication by budding and division; all of which is explained with more or less ingenuity and success. It had an apparent support in several recent and important observations; such as the existence in certain seeds, of an embryo plant, the multiplication by interior germs of *volvox globator*, the reproduction of aphides without impregnation, and Haller's researches on the formation of the embryo chick. Haller himself defended it from a variety of objections, and adduced a number of arguments in its favor.[2] But the theory itself went so far beyond the limits of possible demonstration, that its basis of reality bore no calculable proportion to its towering superstructure. It discounted the results of embryological discovery at too exorbitant a rate; and it could not afford to have any of them turn out differently from what was expected. This happened in the case of the mammalian egg. Subsequent observations, made with better facilities, showed that the organs of the fœtus do not pre-exist in the ovum, in the sense imagined by Bonnet. The impregnated ovum has, it is true, a remarkable property, by which each separate part assumes afterwards a definite shape, and becomes a distinct organ. But the actual configuration of these organs is certainly not present in the ovum, because we can now see its original structure, as well as the intermediate forms which are to be replaced by those of the future embryo. The system of the inclusion of germs, in the time of its glory, embraced the whole history of the world from beginning to end, and the mechanism of growth and reproduction throughout the organized creation, it claimed for its support the results of scientific discovery, and of logical deduction; but it came to grief when the study of embryology had reached a certain stage in its development, and had demonstrated the actual constitution of the mammalian egg.

DILATATION OF THE STOMACH AS A DIAGNOSTIC MEASURE.—Dr. ROSENBACH suggests, in the *Deutsche Med. Wochenschrift*, No. 2, 1882, the distension of the stomach with carbonic acid developed from the action of the Seidlitz powders, taken separately, as a valuable means of examining the condition of the stomach and as a fixed boundary for study of the abdomen.

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[1] Œuvres de Bonnet, Tome iii., p. 140.

[2] Haller, Elementa Physiologiæ Corporis Humani, 1766. Tome viii, Sect. ii; Art. xxix.

ON THE

INFECTIOUS CAUSES OF INFLAMMATION.

Two Lectures delivered at the Pathological Institute of the University of Leipsic, December 8 and 9, 1881.

BY PROF. JULIUS COHNHEIM.

LECTURE II.

(Reported for the MEDICAL NEWS, by special permission of the Author, by Dr. WALTER MENDELSON.)

GENTLEMEN: We were speaking, yesterday, of the investigations of Koch and others into the nature of anthrax. Let us now continue the subject by considering some of the causes of other infectious diseases. Though we have progressed very far into the knowledge of the virus of anthrax, the investigations of the causes of the great majority of the other infectious fevers have so far not been crowned with a like success. It is true that in a number it has been possible to find minute organisms in the blood of those affected, but in no other case (if we except trichinosis) can we trace the whole connection between these microphytes and the disease, as has been done in anthrax. A decade has elapsed since Obermeir found in the blood of patients suffering from relapsing fever certain wavy, thread-like bodies, the *spirillæ*. Their presence in the blood was observed to correspond to the relapses of the fever, the febrile state persisting so long as they remain and becoming less intense as they diminish in numbers, while during the intermissions no spirillæ can be found. Koch and others have made experiments with a view to propagating them, but so far without results. In typhoid fever, Klebs and Eberth have found a bacillus, which they claim to be the specific virus. You all know that micrococci have been found in the intestines and dejecta of typhoid-fever patients for a long while; but I need hardly tell you that such exist in the intestines and fæces of healthy persons, and are of no significance. These two investigators, however, have succeeded in finding in the lymphatic glands and vessels in the vicinity of the typhoid ulcers, rod-shaped organisms, bacilli, which are somewhat smaller than those of anthrax. Klebs and Tommasi-Crudeli, of Rome, have found a somewhat longer bacillus in patients suffering from malarial fever, and they have not only found them in the patients, but in the soil of the malarious districts. In lepra, Neisser, of Leipsic, has discovered a bacillus existing in the nodes on the skin, which is very much smaller than any of the foregoing, requiring a very high magnifying power to become visible. He found them quite constantly, lying closely packed together, and to their increase he attributes the growth and distention of the nodes. Many years ago, Hansen, a Norwegian, described what he supposed to be the specific leprosy germ; but what he saw were simply micrococci, and certainly, by the methods he used, it is impossible to demonstrate the bacilli found by Neisser. Neisser has further discovered the specific virus of gonorrhœa, this time a micrococcus; and Klebs has published at various times the existence of microphytes in a number of infectious diseases, among which I might mention syphilis and lobar pneumonia; and although other investigators have not always had the same success in finding them, and thus confirming their existence, still, any statement coming from this excellent authority must be looked upon with the highest consideration.

Opposed to those who seek for each infectious disease its virus in a well-defined and separate germ, there are a certain number who deny the specific nature of these pathogenic fungi, but hold that the same species, differing in form during different generations, both morphologically and physiologically, may cause the different infectious diseases. At the head of this school stands Nägeli, of Munich, who with his pupils has labored hard to uphold this theory; a theory which I hope to be able to show you later, gentlemen, will not bear the test of close critical investigation. Even though this view were correct, the parasitic hypothesis of the nature of infectious diseases would of course be in no wise affected; still, it is a question too pregnant of interest to be passed over in silence. If we consider the question of the changeability of the pathogenic organisms, surely you must agree with me that, from the point of clinical experience alone, we must reject such a view. We know that there is no class of diseases more definite in their course than the infectious ones, none whose individuality is more clearly stamped upon them. As far back as we have any trusty records at all, measles has always been described with the same symptoms that it has to-day; scarlet-fever the same; and so on I might multiply instances; and although it is true that in certain epidemics some symptoms appear more prominently than others, and even that in the course of centuries, certain infectious diseases have become modified in some particulars, still, it is unheard of that these diseases should have changed any of their characteristic features, or that one should even have become transformed into another. There exists not one reliable record of the contagion of measles producing aught but measles, or that a small-pox patient ever infected any one with anything but small-pox; and though there have been attempts, from time to time, to show that measles in one has produced scarlet-fever in another, and *vice versa*, yet, in the few cases which have been adduced in support of this assumption, it has usually been a question of the rash of measles becoming so confluent as to resemble that of scarlatina. Similarly, typhus and typhoid have been compared, but it seems to me that their only points of resemblance lie in their names.

Now, if it were really true that the different infectious diseases were caused merely by different forms of the same fungus, it would certainly be justifiable to suppose that the changes from one form to another might take place in the patients themselves, and that a case beginning, say as measles, should end as scarlet-fever or as diphtheria, or some other zymotic disease, or, at least, that an epidemic should change its nature in the course of its career. It is, however, almost superfluous to say that such an occurrence has never been noticed.

Conclusive proof of the mutation of form could only be furnished by actual observation and experiment on the schizomycetes themselves, and this Buchner has tried to afford. A work recording his observations appeared last year, and, as it contained some apparently startling revelations, it created a great sensation at the time. By repeated propagations he affirmed that the highly virulent *bacillus anthracis* could be changed into the harmless hay bacillus, two microphytes whose appearance and metamorphoses exactly resemble each other. Were this true, it would indeed be a striking and most important fact, but, unfortunately, his observations have not been confirmed by other investigators.

It is only a few weeks ago that the results of the labors of Koch and some of his pupils have been published, which entirely and conclusively upset Buchner's statements. They show that Buchner did not observe the needful precautions to eliminate from his experiments all sources of error, for, by observing the necessary care, Koch was able to propagate either bacillus indefinitely, without the slightest change taking place in its nature. Indeed, the very methods which Koch used as far back as 1878, to obtain one kind of bacillus free from admixture of others, go to prove the untenability of Buchner's views. He found that when a putrid fluid from any source was inoculated into the wounds of rabbits, mice, or other animals, one kind of the many schizomycetes originally present

finally gained the upper hand, and produced a certain form of traumatic disease. If another animal was now inoculated from the individual so affected, that form of bacillus was invariably found which had in the first instance gained the ascendancy. If we take these facts, combined with those furnished by clinical medicine, into consideration, I think we will have good grounds for believing that the separate characteristic infectious diseases correspond to equally separate and characteristic forms of fungi, or, to express it a little differently, that each infectious disease owes its peculiarities and typical course to the vital attributes and development of the organism which causes it.

Another thing which speaks very strongly in favor of a differentiation, is the *incubation* stage noticed in infections. In the great majority a certain period of time elapses between the reception into the system of the virus and the appearance of symptoms, and during this period of *latency* or *incubation*, the patients usually feel in good health, or at least show no *specific* symptoms. Still another, and even more remarkable fact, is that in each disease the stage of incubation is of a different length, and that in each it lasts a definite number of days. I really hardly know of any more striking evidence in favor of the parasitic theory of the causes of infectious diseases in general, and specific nature of the separate pathogenic germs in particular, than is given us by this century-old experience. It can certainly not depend on the quantity of virus introduced into the system, that measles invariably appears on the ninth to eleventh day after infection, but rather that the virus needs this period of time in order to develop to a morbific agent. Anthrax only becomes apparent when the spores have developed to bacilli, and the symptoms of trichinosis, the fever and myalgia, make themselves noticeable only after the intestinal trichinæ have developed from those eaten with the infected meat, and from these a new generation has been hatched. The number of trichinæ introduced may modify the degree and intensity of the disease, but never its typical course.

Thus everything seems to point to a *contagium vivum*, against which no other valid objection can at present be raised than that in so many infectious diseases its existence has not as yet been absolutely demonstrated.

If we now apply all that we have been discussing to the infectious forms of *inflammations*, I think we may formulate our knowledge thus: that the alterations in the vascular walls of certain localities, which result in changes in the circulation leading to inflammations, are due to schizomycetes. The reason that these fungi establish themselves, after getting into the system generally, in one particular organ or part of the body, depends, no doubt, on the same reason that the trichinæ born in the intestines are finally found in the muscles, namely, that in no other place do they find a material suitable to their nourishment. However much this assumption may be justifiable, you must still not forget, gentlemen, that it is by no means proved, and affords material merely for a provisional and elementary hypothesis. In what manner the schizomycetes react upon the walls of the vessels, it has as yet hardly been attempted to explain.

In the case of the putrefaction bacteria and the micrococci, we thought the inflammatory changes produced were due to a chemical decomposition of the fluids of the body dependent on their increase and growth. It may be possible that in the case of the specific germs of the infectious diseases, similar chemical changes are produced in the vascular walls of the parts where the germs are localized. If this be really a fact, if it be indeed true that cerebro-spinal meningitis, for instance, is caused by certain schizomycetes finding their necessary nourishment in the pia mater, and here causing, by their growth and development, such chemical changes that the walls of the capillaries of the pia become altered in their structure, it seems to me we are entitled to look upon the metastatic inflammations and abscesses of pyæmia as a particular subvariety of infectious inflammation, and of such a kind, indeed, which distinguishes itself from all other forms of infectious diseases by the fact that it owes its origin to *fungi which seem to exist everywhere and at all seasons*. In this way we can readily explain that although a wound can, in any part of the world, become infected, or that even erysipelas or pyæmia may appear sporadically, as it is termed, yet that these diseases are also at the same time markedly contagious, as only too many instances have proved. The germs exist everywhere, it is true, but the liability of infection increases, of course, if a wound be brought near a person whose body presents a more than usually rich breeding-ground.

We see, therefore, that the old idea that the infectious diseases might appear in any place or at any time can no longer be maintained; yet, after all, this limitation relates to a point of but very subordinate importance, and which has really as little true connection with the nature of the diseases as their classification into contagious, miasmatic, and miasmatic-contagious. For us, gentlemen, the thing of chief importance, in speaking of infectious diseases, is the parasitic nature of their morbific causes, or, as we may also say, the organized nature of the virus, and we know to-day that, aside from exceptional cases, it depends on the biological peculiarities of these parasitic organisms whether they enter our systems by way of the water we drink, the air we breathe, or from already infected individuals. Since these side issues of the general question have been more closely studied, the number of diseases which can be classed as infectious has, in the last decade, slowly but steadily increased, and many are now reckoned in this group whose true etiology was not even suspected a few years ago. To show you how compatible the present parasitic theory is with the always known clinical history, let me merely bring to your minds a familiar instance. That a coryza—a simple catarrh of the nose or throat or larynx, or, as it is generally called, "a cold"—is quite markedly contagious, has long been recognized by both the public in general and by physicians. Yet how could a disorder having such marked powers of reproduction ever be produced by changes of temperature, wind, rain—in short, atmospheric conditions of any kind? A confirmation of the correctness of our views, of still greater weight, has been furnished us by a more close and critical examination of the anatomical and clinical histories of cases. This has shown that many inflammations, formerly looked upon as independent, are in reality directly connected with infectious diseases. In pleuritis and peritonitis, pathological anatomists have maintained for some time this relation. For many cases of endocarditis and meningitis, too, this view obtained. Quite lately Wagner, of Leipsic, basing his statements on his large clinical experience, has come to the conclusion that the great majority of inflammations of the kidney develop secondarily to some infectious disease which has preceded them. Similar proofs have been given for chronic bone and joint inflammations by Lücke, and for the internal inflammations of the eye by Leber. If you will bear in mind, too, that by the progress of pathological anatomy, certain formerly unexplained inflammations, such as the congenital valvular lesions from intrauterine endocarditis, certain lesions of the spinal cord, etc., have been shown to be of a non-inflammatory nature, it will be at once evident to you that the number of inflammations depending on

inexplicable and incomprehensible causes is rapidly diminishing. If for an etiology of inflammation we are not yet in a position to entirely discard the idea of a *rheuma*, that is, an abnormal condition of the skin or mucous membranes or other organs of the body, produced by atmospheric conditions, it seems to me that the science of medicine is at least on the best way to do so.

ORIGINAL ARTICLES.

THE USE OF CONSTITUTIONAL REMEDIES IN THE TREATMENT OF EAR DISEASES.[1]

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THIRTY years ago Sir William Wilde (then Mr. Wilde), of Dublin, in a chapter devoted to the consideration of otorrhœa, in his excellent work upon Aural Surgery, wrote as follows: "With regard to general treatment, a very remarkable difference seems to exist in this country in relation to the management of the diseases of the eye and the ear; that of the former being of too local a nature, while that of the latter is almost exclusively constitutional. How frequently do we see a case of pustular or rheumatic ophthalmia, with a foul, white, loaded tongue, treated by a caustic solution dropped into the eye, or an astringent lotion applied to the eyelids; while a case of otorrhœa, without any derangement of the digestive functions or general health, is recommended tonics, sea-bathing, blistering, and an issue in the arm."[2]

Sir Wm. Wilde was an eminent teacher, and probably as much as any man aided in shaping the development of modern otology, and it may be that this criticism of his upon the exclusively constitutional treatment of ear diseases prevalent at the time he wrote, produced a more radical effect than he intended. However this may be, if he were with us to-day, I feel quite sure that his complaint would be, not that constitutional remedies are used to the exclusion of local ones in the treatment of ear diseases; but, on the contrary, that with pendulum-like swing, we have gone to the other extreme, and that now upon local means too great reliance is placed, while the value of constitutional treatment is almost lost sight of. For, though this distinguished author inveighed against the neglect of local means in the management of affections of the ear, he was at the same time a firm believer in the value of internal medication, and one may find in a single chapter of his thoroughly practical treatise more said in commendation of constitutional remedies than he will discover between the covers of some of the works upon ear diseases written within the past ten years. It is, indeed, true that much of Wilde's constitutional treatment consisted in what nowadays would be regarded as an unjustifiably free use of mercury; and, although he speaks with abhorrence of the reckless manner in which this drug had been administered twenty years before his day, allusion to very sore mouths occurs with somewhat unpleasant frequency in the reports of his cases. But there *are* conditions in which, I believe, much is to be gained by following his example more closely than we do, even in this particular.

The very recently issued works upon diseases of the ear, by Dr. A. H. Buck,[3] of New York, and Mr. Dalby,[4] of London, may be referred to as affording proof of what I have said as to the slight importance attached to the use of constitutional remedies by recent writers upon diseases of the ear. Dr. Buck, it is true, does not ignore constitutional remedies to the same degree that Mr. Dalby does; nevertheless, he damns them with faint praise in eczema of the auricle; speaks only of quinia, iron, and bark in furuncular inflammation, and of Fowler's solution in diffuse inflammation of the external auditory canal, and says of the latter, he has had too little experience with it to express a decided opinion regarding its efficacy; recommends as "general measures" in middle-ear catarrh nothing except change of air, adequate exercise, attention to the diet and cold sponge-baths, and bromides for the relief of tinnitus; suggests nothing beyond local treatment in acute purulent otitis media but rest and a non-stimulating diet, unless it be due to syphilis, when, in addition, "constitutional remedies" should be administered; mentions only iodide of potassium in deafness due to non-specific disease of the labyrinth, but speaks with favor of the "vigorous antisyphilitic treatment" recommended by Dr. Roosa when the labyrinthine affection is of specific character; and, most noteworthy of all, concludes his exceedingly interesting and instructive chapter upon "Diseases of the Mastoid Process" by devoting six pages to the question of "treatment," *without making one single allusion to internal medication.* One might be disposed to conclude that this failure to speak of anything besides local measures, in considering the treatment of mastoid affections, is the result of an oversight upon the part of the author; but the description given of the treatment adopted in the numerous cases of this character which he reports, renders it probable that such is not the case, but, on the contrary, that it is a true index of his convictions.

This seems the more remarkable, because at least one of the cases related illustrates in a striking manner what, under the most unfavorable circumstances, active constitutional treatment is capable of accomplishing. The case I allude to[1] is that of a little girl, six years of age, in whom alarming cerebral symptoms developed in connection with a severe attack of acute inflammation in the drum of each ear. The inflammation had lasted one week when Dr. Buck was called in. He found the patient slightly delirious, with a temperature of 105° and a pulse of 140. There was almost total deafness, and both tympanal membranes were red and swollen, the right one bulging somewhat superiorly and posteriorly. The mastoid region showed no external signs of disease. Dr. Buck made "a free incision" into the posterior half of each membrane, giving vent to no pus, and prescribed only the warm douche, to be used freely, and poultices. On the following day the child was in a state of partial coma; the temperature had not improved; pulse 140, and feeble; sordes upon the teeth; tongue dry; and no discharge from either ear. Drs. Markoe and James R. Wood were now called in consultation, and at their suggestion "mercury was administered: internally, in the form of calomel in small doses; externally, in the form of inunctions of the oleate of mercury (10 per cent.), frequently repeated. Bromide of potassium was also given, in conjunction with the calomel." The next morning, Dr. Buck goes on to relate, the child was "very much better, an active discharge having established itself in both ears during the night;" and "from this time onward"—as he subsequently learned from her physician—"she made a rapid recovery, and regained her hearing almost perfectly." The lesson which this case teaches is so plain, it seems to me, that he who runs may read. Dr. Buck, however, does not appear to

[1] A paper read before the Clinical Society of Maryland, January 6, 1882.

[2] Practical Observations on Aural Surgery. Philadelphia, 1853, p. 390.

[3] Diagnosis and Treatment of Ear Diseases. New York, 1880.

[4] Diseases and Injuries of Ear. Philadelphia, 1873.

[1] See page 307 of Dr. Buck's book. The case occurred in January, 1876.

have been impressed by it; at least, the method of treatment so successful in this instance was resorted to by him in but one other case,[1] and then only in the last extremity, when from the "advanced stage of the cerebral disease" and the absence of redness and tenderness behind the ear, "it seemed unlikely that any relief would be afforded by the operation of perforating the mastoid process." As may be supposed, the mercurial treatment here, was of no avail, and the patient died two or three days later.

With each succeeding year of experience, I have learned to place a higher estimate upon the value of internal medication in the treatment of diseases of the ear, until at the present day I resort to constitutional remedies with nearly the same frequency, and almost the same confidence in aural, that I do in ophthalmic practice. And here I may remark, in parenthesis, that this result is due in no small degree to my experience in dealing with eye diseases; for, assuredly, nowhere do we find more distinct and convincing proofs of the efficacy of constitutional remedies than in ophthalmic practice.

At the head of the list of remedial agents in which experience has taught me to place confidence in diseases of the ear, I do not hesitate to put the once much abused in practice, and later scarce less abused in precept, drug —*mercury*. It has not been very many years since mercury was freely employed in the treatment of ear diseases. Wilde, as we have seen, used it with great freedom, and Toynbee also prescribed it very often. Of late, however, the tendency has been to resort to it only when there is reason to believe that the local malady is dependent upon or complicated by *syphilis*. "Mercury," says Sir Wm. Wilde, "is the medicine which, of all others, acts most beneficially in diseases of the ears, simply on account of its specific efficacy in arresting or controlling inflammation or removing its products."[2] And if we admit, as I am quite prepared to do, that mercury has such "specific efficacy," we need only call to mind the minute anatomy of the ear—the peculiar character of the membrane, mucous membrane and periosteum, at once, which lines the tympanal cavity and mastoid cells, and of that, fibro-serous in structure, which invests the intricate walls of the labyrinth—to appreciate that here, of all places, in an inflammation involving such structures, a peculiarly favorable field is open for its action.

Of the value of mercury, more especially the bichloride, *in chronic and subacute inflammation of the middle ear*, Wilde speaks in the most positive terms;[3] while Roosa, on the other hand, with equal positiveness condemns its employment under such circumstances, and says, "the time has probably gone by when a person in fair health, suffering from chronic aural catarrh, and who has no constitutional taint, will be treated by alterative doses of the bichloride of mercury, followed by the iodide of potassium."[4] Although I have the highest regard for Dr. Roosa's opinions, and esteem him a thoroughly practical surgeon, I should be extremely sorry to think that the future would verify this prediction of his, for my own experience leads me to endorse heartily the views of Wilde, and to believe *that in giving up mercury we should lose the one remedy which we can least afford to do without.* And I may add that I am also prepared to endorse what he has said in the same connection as to the perfect safety which attends even the long-continued administration of the bichloride of mercury, when it is prescribed in doses proportioned to the susceptibility of the patient. The *biniodide*, however, is the preparation of mercury which I more frequently employ, in both ophthalmic and aural practice. The addition of two grains of iodide of potassium to each grain of the biniodide of mercury renders it freely soluble in water, and in this form I usually prescribe it, giving, according to circumstances—the susceptibility of the patient and the nature of the malady—from $\frac{1}{48}$ or $\frac{1}{60}$ of a grain to $\frac{1}{10}$, $\frac{1}{12}$, $\frac{1}{8}$, or even in some instances $\frac{1}{6}$ of a grain of the mercury salt, three times a day.[1] In deafness due to middle-ear changes, unless I have reason to think the patient will be unusually sensitive to its action, I commonly begin by prescribing $\frac{1}{32}$ or $\frac{1}{24}$ of a grain, to be taken three times a day, after meals; and very often when only slight, or perhaps no appreciable improvement follows its administration in these doses, together with careful local treatment, which should never be neglected (but into the consideration of which the limits of this paper do not permit me to enter), a marked change for the better *does* become manifest *when the dose is increased to* $\frac{1}{16}$ *or perhaps* $\frac{1}{12}$ *of a grain.* Occasionally, in persons who are peculiarly susceptible to the action of iodine, I substitute with advantage, and in similar doses, the *bichloride of mercury* for the biniodide solution; and, on the other hand, I sometimes increase the proportion of iodide of potassium in the latter, so that a full dose of it may be given with the mercury.

The employment of mercury is especially indicated, and its good effects are more manifest in that variety of middle-ear catarrh so frequently encountered, which least deserves to be called *catarrh*, in which the tendency is towards sclerosis and thickening, and in which the fibrous structures of the tympanum are, perhaps, chiefly involved. Often, however, even in true mucous catarrh, we shall accomplish more with it than with any other drug, although in this condition I usually employ, instead, the muriate of ammonia, of which I shall presently speak.

It is, perhaps, scarcely necessary for me to state here, and yet I think it will be more discreet to do so (for there is nothing leads so surely to circumspection, and this to introspection, as a little wholesome criticism), *that I have not confounded syphilitic and non-syphilitic cases of middle-ear disease*, and that what I have said, so far, as to the value of mercury in chronic and subacute tympanal catarrh has reference *only to cases of non-specific character.* When, as we so often find, these middle-ear maladies *are* dependent upon constitutional syphilis, inherited or acquired, then, of course, all that has been said as to the propriety of administering mercury, and the good which it is capable of accomplishing, may be repeated, as to them, with twice the emphasis. In this connection I may state that my uncle, Dr. Alan P. Smith, whose opinion is valuable because of his large experience and habit of observing closely,

[1] Op. cit., p. 320, Case XIV.
[2] Diseases of the Ear, p. 98.
[3] Ibid, p. 210.
[4] Treatise on Diseases of the Ear, New York, 1873, p. 289.

[1] At the present time I have a man (a German, 46 years of age, by occupation a shoemaker) under treatment for neuro-retinitis, who, since November 2, 1881, has taken *every week four grains* of the biniodide of mercury ($\frac{1}{24}$ gr. three times a day) with the happiest effect as regards his vision, and without the slightest disturbance of his general health or interference with his daily occupation. He had been taking strychnia in full doses for some time before coming into my hands in January last; but, according to his report, with no benefit to his sight. He was allowed to continue the strychnia, and, in addition, was given the biniodide, in the solution above described, $\frac{1}{8}$ grain to be taken three times a day. When this treatment was commenced, he could with difficulty decipher words of J. No. 12. Subsequently the dose of biniodide was increased upon several occasions, until, upon the date mentioned, the present quantity was reached. With each increase in the dose (there being no change in the strychnia dose), a more marked improvement in vision became manifest, until at the present time he can make out short words of J. No. 1, and read No. 2 with tolerable fluency. He denies syphilis, and there seems to be no good reason for considering the affection of specific character. This is, in my experience, an instance of exceptional insusceptibility to the action of mercury, although Wilde speaks of giving ¼ grain of the bichloride for "months together" without unpleasant consequences.

tells me that in chronic nasal catarrh, even when it occurs independently of syphilis, he has obtained more satisfactory results from the long-continued administration of small doses of the biniodide of mercury than from any other plan of treatment which he has tried.

In *inflammation involving the nervous apparatus of the ear*, whether of specific or non-specific origin, mercury, in the form of the biniodide is, in my opinion, by far the most valuable remedy that we possess. In opposition to the views of a recent writer upon this subject, I believe that syphilis, both inherited and acquired, *very frequently invades the labyrinth*, although, undoubtedly, the middle ear is at the same time often (but not always) involved. If I were asked to state what, in my opinion, are the most frequent causes of nervous (labyrinthine) deafness, I think I should have no hesitation in answering: first, cerebro-spinal meningitis; next, *syphilis;* and, after that, traumatism. I have not had the good fortune to see any cases of deafness from cerebro-spinal meningitis improve under treatment; possibly some of those which I have met with might have been benefited by mercury if they had been seen at an earlier period. That very much may be accomplished in syphilitic disease of the labyrinth, sometimes even when the deafness is of considerable duration, by the persistent administration of moderate doses of the biniodide of mercury, I have convinced myself in a number of instances. I have also obtained better results from this remedy in Ménière's disease, uncomplicated by syphilis, than from anything else that I have employed.

It is a question in my mind how far the administration of mercury is justifiable *in that form of acute inflammation of the tympanum which, if allowed to run its course unchecked, usually ends in suppuration, in perforation of the drumhead, and in the establishment of a more or less persistent otorrhœa.* Practically, as these cases seldom come into the hands of the specialist until after the period is passed at which this plan of treatment might be resorted to with advantage, he is not often called upon to decide this point. Of course, in this condition, alterative doses of the biniodide or bichloride would be of little avail, and, so far, I have not met with cases in which I have felt called upon to administer calomel or blue mass so as to impress the system. I have, however, given purgative doses of calomel in combination with rhubarb, or with rhubarb and scammony, in acute inflammation of the middle ear with the happiest effect. And I believe that when the inflammation is intense, becoming a *periostitis* rather than a catarrhal inflammation of a mucous membrane, when there is marked constitutional disturbance, and, above all, when symptoms of cerebral irritation make their appearance, I believe, that under these circumstances, considering how great is the danger of meningitis supervening, it is our duty, without loss of time, and in addition to local antiphlogistic treatment (of which the free application of leeches will form a chief part), to bring the patient quickly under the influence of mercury; and this we shall accomplish best by administering at short intervals small doses of calomel, combined, if need be, with opium, and occasionally, perhaps, by supplementing this with inunctions of the oleate or the officinal ointment of mercury. For is it not to be desired rather that the patient should recover, somewhat "pulled down," it may be, by the active treatment he has received, but with a sound ear, than that he should be suffered to die from meningitis or cerebral abscess, or, at best, get over the acute attack with a permanently damaged ear, and a persistent otorrhœa to remind him of what he has gone through with, and to be a standing menace to the integrity of his brain for the rest of his days?

(To be continued.)

ON THE ANTIPYRETIC TREATMENT OF TYPHOID FEVER.

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THE following are the tables referred to in the lecture on the antipyretic treatment of typhoid fever, contained in the first number of the MEDICAL NEWS (January 7, 1882). They embrace the data on which were based the conclusions presented in that lecture. In each table will be found

1. The number of times, during the continuance of the disease, that the different modes of refrigeration, namely, the ice-bath, the sponge-bath, the wet sheet, with sprinkling, were employed.

2. The temperature in the axilla whenever either of these modes was employed.

3. The duration of the employment of each of these modes whenever employed.

4. The temperature in the mouth when the employment, at each time, was concluded.

5. The duration of the antipyretic effect in every instance.

The number of tables, corresponding to the number of cases, is fifteen. It is needless to say that the records made of the cases, and the tabulation of the facts, have required considerable labor, which, it is hoped, will not be without value as data having reference to the antipyretic treatment of typhoid fever.

CASE NO. 1.—RECOVERY.

Modes of Employment of Cold.	Axillary Temperature when Commenced.	Duration of Employment.	Temperature in Mouth at Conclusion.	Duration of Antipyretic Effect.
Ice-bath.	105°	1½ hrs.	103¾°	1½ hrs.
"	105°	½ hour.	102°	2 hrs.
"	105°	1½ hrs.	103°	1 hour.
"	105°	½ hour.	102½°	3 hrs.
Sponge-bath.	105°	6 hrs., at intervals.	102 1-5°	Not noted.
Ice-bath.	105¾° in rectum.	Not noted.	Not noted.	Not noted.
Sponge-bath.	104 1-5°	3½ hrs.	102°	1 hour.
"	103 1-5°	1½ hrs.	103 1-5°	Not noted.
"	104°	1 hour.	103¾°	Not noted.
Sponge-bath.	103½°	6 hrs.	102¾°	

Subsequently, in this case, 7 sponge-baths were given on the next day; 7 the next day; 8 the next day, and 5 the next day. After 7 days 32 baths were given in one day, and 2 on the day following. After 21 days, 3 baths were given during one day; 2 daily on the next 3 days, and 3 on the next day. These subsequent baths are not tabulated, for the reason that in many instances the time of their continuance is not noted.

The number of ice-baths given in this case is 5, and the number of sponge-baths 46.

CASE NO. 2.—RECOVERY.

Ice-bath.	103¾°	15 minutes.	101°	1½ hours.
"	104°	An hour.	102°, and subsequently in 2½ hrs. 101°	15 hours.
Sponge-bath.	103°	Not noted.	101°. 5 h. after conclusion, 99°	6 hrs. after reduction to 99°
"	102¾°	"	102°	
"	103½°	"	102°	12 hrs. after reduct'n to 102°
"	104°	"	101½°	

In this case, 7 additional sponge-baths were given, the details not having been noted.

The number of ice-baths is 2, and the number of sponge baths is 11.

CASE NO. 3.—FATAL.

Wet sheet.	104°	One hour.	102¼°. An hr. and a quarter after conclusion 101°, and after another hour 100°.	Four hours.

In this case pneumonic symptoms were present on the day but one following the employment of the wet sheet, and death took place four days afterwards. The temperature rose to 108° on the day of death.

CASE NO. 4.—RECOVERY.

Wet sheet.	103½°	2 hrs.	103°. An hour and a half after conclusion, 102¼°; and in 4 hours, 100¾°.	No rise of temperature except for an hour once during ten days.
"	103½°	2 hrs.	102½°	1 hour.
"	103½°	4 hrs.	102°	48 hours.
"	103½°	4 hrs.	102½°	24 hours.
"	103¼°	4 hrs.	102½°	No further rise of temperature to 103°.

CASE NO. 5.—RECOVERY.

Wet sheet.	104½°	4 hours and 45 minutes.	101¾°	2 hours.
"	105½°	6 hours.	103½°	1 hour.
"	105¼°	6 hours.	104°	Owing to delirium and complaining by the patient, cold not employed for 18 hours.
Sponge-bath.	105°	2 hours.	103¼°	Afterward the temperature did not call for antipyretic treatment.

CASE NO. 6.—RECOVERY.

Wet sheet.	104¾°	1 hour.	102¼°	2 hours.
"	104¾°	1 hour.	102°	3 hours.
"	104°	2 hours.	102°	1 hour.
"	103¾°	1 hour.	Not noted.	Not noted.
"	104°	2 hours.	101¼°	2 hours.
"	104¼°	1½ hrs.	101¾°	1½ hrs.
"	105½°	2 hrs. and 45 mins.	101½°	2½ hrs.
"	104°	2 hours.	101½°	1 hour.
"	103°	50 mins.	102°, and in 2 hrs. after conclusion, 101¼°.	6½ hrs.
"	103½°	30 mins.	101¼°	5 hours.
"	103°	45 mins.	101¾°	4 hours.
"	103¾°	25 mins.	101¾°	2½ hrs.
"	103¾°	1 hour.	101¼°	13 hours.
"	103½°	45 mins.	101¼°	3 hours.
"	103¾°	35 mins.	101°	20 hours.
"	103¾°	40 mins.	101°	5 hours.
"	103¼°	1 hr. and 25 mins.	101½°	Subsequently no rise of temperature to 103°.

The wet sheet was employed in this case 13 times.

CASE NO. 7.—RECOVERY.

Wet sheet.	104½°	20 hours.	103½°	Wet sheet discontinued, owing to antipathy of patient.
Sponge-bath.	103½°	6 hours.	102¾° in axilla. 14 hours after sponge-bath 101¼°.	4 hours.
"	103¼°	Not noted.	Not noted.	Not noted.

In this case the temperature several times afterward rose to 104°, but was always reduced by the sponge-bath.

CASE NO. 8.—FATAL.

Sponge-bath.	104¾°	30 minutes.	102¾°	9 hrs. 103¾°, and 16 hrs. 104°.
"	104 1-5°	20 minutes.	102½°	9 hrs. 103¾°, and 18 hrs. the same.
"	103¾°	20 minutes.	101¼°	2 hours.
"	104¾°	20 minutes.	102°	2½ hrs.
"	104½°	20 minutes.	101¾°	9 hrs. 103½°, and 12 hrs. 104°.
"	103½°			

The patient became comatose, and died on the second day after discontinuance of the antipyretic treatment.

CASE NO. 9.—RECOVERY.

Sponge-bath and rubbing surface with ice for ten minutes.	104⅛°	1 h. and 10 min.	102°	2 hours.
Sponge-bath.	104°	25 minutes.	Not noted.	3 hours after sponge-bath.
"	104¾°	45 minutes.	103¼°	Rubbed with ice immediately.
Rubbing surface with ice.	104⅛°	101¾°	101¾°	2 hours.
Sponge-bath, and rubbing with ice 20 minutes.	103¼°	30 and 20 min.	101½°	In 5 hours 103°

In this case, for 5 days after last sponge-bath and rubbing with ice, the temperature was generally under 103°, and cold was not employed. Then the temperature rose to 103½°, and the sponge-bath in 20 minutes reduced it to 102¾°. Subsequently, the sponge-bath was employed 13 times, and the surface repeatedly rubbed with ice, always with reduction of the temperature.

CASE NO. 10.—FATAL.

Sponge-bath.	104¾°	20 min.	102°	2½ hours.
"	104¾°	30 min.	No reduction of temperature.	
Wet-sheet.	104¼°	2 hours.	102½°	8½ hours.
"	104¾°	8 hours.	103°	2 hours; temperature 105°; and in 6 hrs. fell to 101°, without any antipyretic treatment. In 5 hrs. rose to 104°, and an hour later to 105°.
Sponge-bath.	105°	1 hour.	102¾°	6 hours.
"	105°	2 hours.	103°	3 hours.
"	105°	2 hours.	102¾°	13 hours.
"	104°	3 hours.	102¾°	3 hours.
"	105°	1½ hrs.	102¾°	2½ hours the temperature was 105°, and in 1½ hours fell to 102¾°, without antipyretic treatment. In 36 hours afterward it rose to 104°.
"	104°	1½ hrs.	102¾°	20 hours.
"	105°	8 hours.	102¾°	15 hours.
"	105⅛°	11 hrs.	103°, and 2 hours after 102°.	In 11 hrs. the temperature rose to 105°, and in 4 hours fell to 103¾°, without antipyretic treatment. In 18 hours rose to 104°. Death 11 hrs. afterward, the temperature remaining at 104°.

CASE NO. 11.—FATAL.

Wet sheet.	104¾°	3 hours.	102¾°	23 hours.
Wet sheet in "Kibbe's cot."	103¾°	1½ hours.	103°	26 hours.
"	103¾°	5 hours.	102¾°	13 hours.
"	103¾°	4½ hours.	102½°	16 hours.
"	103¾°	45 minutes.	102½°	6 hours.
"	103½°	1 hour.	102°	2 hours.
"	103½°	3 hours.	102¼°	5 hours.
"	103¾°	4 hours.	102½°	6 hours.
"	104¾°	7 hours.	102¼°	4 hours.
"	103½°	2½ hours.	102°	5 hours.
"	103¾°	4 hours.	102½°	3 hours.
"	104°	17½ hours.	103°	4½ hours.
"	104¾°	2 hours.	102¼°	3 hours.
"	103¾°	3 hours.	102°	4 hours.
"	104°	3 hours.	102¼°	4 hours.
"	104°	3 hours.	102°	2 hours.
"	103¼°	3 hours.	102½°	4 hours.
"	103¾°	7 hours.	101½°	14 hours.
"	103½°	2 hours.	102°	80 hours.
"	103¾°	1 hour.	102½°	4 hours.
"	104¼°	3 hours.	102½°	4 hours.
"	104°	2 hours.	102¾°	12 hours.
"	103¾°	1 hour.	102¼°	4 hours.
"	104°	3 hours.	103½°	4 hours.
"	103¾°	3 hours.	102°	15 hours.
"	103½°	1 hour.	102½°	Quinia substituted and bath suspended for 24 hrs.
Sponge-bath.	104¼°	2 hours.	102½°	
Sponge-bath and 20 grs. quinia.	104¼°	3 hours.	102°	21 hours.
"	104°	1 hour.	103¼°	
"	103¾°	12 hours.	101½°	12 hours.
Sponge-bath and 15 grs. quinia.	103¾°	2 hours.	102¼°	2 hours.
Sponge-bath and 20 grs. quinia.	103¾°	2 hours.	102¼°	14 hours.
Sponge-bath.	103°	2 hours.	102½°	4 hours.
Sponge-bath and 20 grs. quinia.	104°	1 hour.	103½°	1 hour.
Sponge-bath.	104⅛°	1 hour.	102⅛°	2 hours.
Sponge-bath and 20 grs. quinia.	103⅛°	4 hours.	102¾°	11 hours.
"	103¾°	2 hours.	101°	6 hours.
"	103¾°	9 hours.	102½°	3 hours.
Sponge-bath.	103¾°	1 hour.	103°	3 hours.
Sponge-bath and 20 grs. quinia.	104°	4 hours.	103°	4 hours.
Sponge-bath.	103¾°	2 hours.	103°	6 hours.
Sponge-bath and 20 grs. quinia.	104¼°	3 hours.	103°	3 hours.
Wet sheet in "Kibbe's cot."	104¾°	2 hrs. and 25 minutes.	102¼°	3½ hours.
"	104½°	2 hours.	102½°	8 hours.
Sponge-bath and 15 grs. quinia.	104¼°	1 hour.	103°	3 hours.
Wet sheet in "Kibbe's cot."	104¾°	2 hrs. and 25 minutes.	102¼°	4 hours.
"	104⅛°	2 hours.	102½°	3 hours.
"	103⅝°	1 hour.	102¾°	16 hours.
"	103⅛°	4 hours.	102⅛°	

Death took place 4 hours after the last employment of the wet sheet, the temperature just before death being 103°. The wet sheet was employed 32 times and the sponge-bath 17 times.

CASE NO. 12.—RECOVERY.

Sponge-bath.	103⅛°	35 min	102⅛°	2 hours.
"	104°	40 min	103½°, and afterward declined to 99½ (3 hours).	8 hours.
"	105°	1 hour	102°	3 hours.
"	103¾°	45 min	103°, and in 2 hrs. afterward fell to 102¼°.	Subsequently no rise of temperature to 103°.

CASE NO. 13.—RECOVERY.

Sponge-bath.	103⅛°	3 hours.	102°	13 hours.
"	103⅛°	2 hours.	102¼°	9 hours.
"	103°	1 hour.	101⅛°	12 hours.
"	103¼°	3 hours.	102°	2 hours.
Wet sheet.	103¼°	11 hrs.	At end of one hr., 104°; continued for 3 hrs. longer, 102½°; continued for 7 hrs. longer, 102°; and in 1 hour after removal of sheet, 101°.	No further rise of temperature to 103°.

CASE NO. 14.—RECOVERY.

Sponge-bath.	104°	1½ hrs.	105⅛°. The wet-sheet then substituted.	
Wet sheet.	105⅛°	6 hours.	102¼°. 3 hours after removal of sheet, 99¾°.	40 hours.
"	104°	4½ hrs.	102°	No further rise of temperature to 103°.

CASE NO. 15.—RECOVERY.

Wet sheet.	103¼°	10 hours.	At end of 3 hours, 105¾°. In 7 hrs., 102°.	6 hours.
"	103½°	6 hours.	At end of 1 hr., 104°. At end of 10 hrs., 101¾°.	15 hours.
"	103°	1 hr. and 45 min.	102¼°. In 15 min. after removal of sheet, 101¾°.	8 hours.
"	104°	4 hours.	102¼°. In 2 hours after removal of sheet, 101°.	No further rise to 103°.

HOSPITAL NOTES.

HOSPITAL OF THE UNIVERSITY OF PENNSYLVANIA.

A SERIES OF CASES OF ANEURISM.

(Reported by WILLIAM E. HUGHES, M.D., LATE RESIDENT SURGEON.)

(*Concluded from page 101.*)

CASE IV. *Aneurism of External Carotid Artery.*—Service of Dr. AGNEW. Mrs. Serina M. P., white, æt. thirty-eight; admitted June 25, 1881. Four years before she had received an injury in the neck near the left angle of the jaw, by being thrown from a carriage. The injury apparently consisted of a slight abrasion and contusion, and nothing was thought of it at the time. Three weeks afterwards, a small tumor was felt at the site of the contusion, which, however, gave her no pain or uneasiness. It grew rather rapidly, and soon the characteristic signs of an aneurism became manifest. Two years after the appearance of the aneurism, an artery in the neck (probably from the patient's description of the operation, the external carotid), was ligated, which, for a short time, arrested the pulsation and bruit. They recurred, however, after a short interval, and the tumor continued to grow. A short time before admission she had several hæmorrhages from the mouth, which were easily controlled by the application of styptics to the bleeding points. When admitted, the tumor extended from the ear to the angle of the mouth, measuring eight inches in that direction; had pushed in the cheek; encroaching on the cavity of the mouth, and pressed on the jaws to such an extent as to render the taking of any but liquid food a matter of great difficulty. Pressure on the left common carotid

artery controlled pulsation perfectly. Dr. Agnew tied the left common carotid June 29, 1881. The tissues were found much matted together, as if by previous inflammation, and it was with great difficulty that the vessel could be separated from its sheath. After the ligature had been placed around the common carotid below the omo-hyoid muscle, it was found that pulsation in the aneurism, though lessened, was not entirely controlled. The incision was carried higher, and the trunk of the external carotid bared. In cutting down to the artery, several large veins were opened. Two silk ligatures were passed around the artery; these included the external carotid, the superior thyroid, lingual and facial arteries, and the bleeding veins. This last ligation controlled the pulsation perfectly. For the first three days after the operation, the tumor remained quiet and relaxed, no pulsation or bruit could be detected. There was perceptible lessening of the swollen veins of the forehead, and the measurement from ear to angle of mouth was one-half inch less than it had been. At the end of three days pulsation and bruit became manifest, and it was evident that blood was finding its way into the sac by means of the other carotid and circle of Willis. Pressure on the left carotid no longer controlled the pulsation, but pressure on the right did so perfectly. To exclude the current of blood flowing into the sac through the right common carotid, this vessel was tied by Dr. Agnew, August 6, 1881, thirty-eight days after the ligation of the left. The same matted condition of the tissues was found as was present on the left side of the neck, the superficial arteries were much enlarged, and several of them had to be tied before the carotid was reached. The silk ligature was passed around the artery above the omo-hyoid muscle; it was tightened slowly, several minutes being employed, in order to cut off the supply of blood to the brain as gradually as possible. The patient reacted well; there were no cerebral symptoms manifested, except a partial loss of memory and slight incoherence in the expression of her thoughts. These lasted about three days, when the mind became perfectly clear, after which she complained that objects were not sharply outlined. The failure of vision remained about the same till her death, but was not at all marked. No ophthalmoscopic examination was made. For the first week the tumor remained quiescent; at the end of that time a bruit became audible, very much fainter, however, than the original one. There was no pulsation; the wound did well, the ligature coming away on the twelfth day. The slight bruit persisted though the tumor, diminishing decidedly in size, measured six and a half inches from the ear to the angle of the mouth, and encroached so much less on the mouth, that the jaws could be much more widely separated than before, and solid food taken with ease. On September 4, a small ulcer, apparently aphthous in character, appeared in the mucous membrane of the mouth, over the prominence produced by the aneurism. A few days later, a gush of blood came from this ulcer, but was easily controlled by plugging the cavity with dry lint. On September 9, a much more serious hæmorrhage occurred, but it too was at length controlled by additional plugging. There was no recurrence of hæmorrhage, but four days later a fetid, sanious fluid commenced to ooze through the plugging. The plugging was removed without the occurrence of any hæmorrhage; symptoms of septicæmia set in, and she died September 15, 1881, eighty-four days after the original ligation.

CASE V. *Femoral Aneurism.*—Service of Dr. AGNEW. Joseph M., white, æt. thirty; admitted August 29, 1881. Patient was a moulder, having much heavy lifting to do, and often carrying heavy loads pressing on his groin. He had not had syphilis. About a year before, while pushing a heavy casting, his feet slipped, and something gave way in his left groin. Shortly afterwards, he noticed a small pulsating tumor. On admission, the aneurism had attained quite a large size. It was on the left femoral artery, apparently originating immediately beneath Poupart's ligament, and extending down the leg five inches. The leg was swollen and œdematous, and could be moved only with intense suffering. Owing to the weakened state of the patient, and his inability to endure the pressure of an abdominal tourniquet, ligation of the external iliac artery was determined on. On dissecting down to the artery, Dr. Agnew found that the aneurism had pushed up Poupart's ligament, and had involved the trunk of the external iliac artery for a distance of an inch above the ligament. A carbolized animal ligature was passed around the trunk of the external iliac, and the wound closed. The wound healed by adhesion throughout one-half of its extent, the other half remaining open and suppurating freely. The aneurism remained perfectly quiescent for about a week; at the end of that time, a faint pulsation, but no bruit, appeared, lasting for a few days, when it ceased, and the tumor remained quiet. The swelling of the leg disappeared in the course of two weeks, and the patient was able to move without pain. The wound healed slowly, and is now almost closed. The tumor contracted gradually, the leg below the tumor resumed its natural appearance, and the patient is now practically well. The tumor, however, remains soft, and there may still be some danger of suppuration.

CASE VI. *Femoral Aneurism.*—Service of Dr. ASHHURST. Wm. S. D., colored, letter-carrier, æt. thirty-nine; admitted October 1, 1881. He could not ascribe the origin of the aneurism to any injury; he had not had syphilis. Thirteen months before admission he noticed a small hard lump, about as large as the end of his little finger, at the apex of Scarpa's triangle in the right leg. At first it gave him no trouble, but about four months after its appearance, he began to have slight pain in it, and the leg grew gradually weak, so that he was unable to go on with his employment. On admission, an aneurism, about as large as a small walnut, was found seated on the right femoral artery at the apex of Scarpa's triangle; there was marked pulsation, but no thrill, and only a very indistinct bruit. Pressure on the femoral artery above the aneurism, stopped the pulsation completely, and the tumor became smaller, and very soft and flaccid, showing that there had been little, if any, deposition of laminated clot. The pulsation in the aneurism being thus easily controlled by pressure, it was determined to try digital compression. Pressure on the femoral artery below the origin of the profunda femoris, was commenced in the evening, and continued, uninterruptedly, for ten and a half hours, when a horseshoe tourniquet was substituted for the finger. Pressure by the tourniquet was maintained about twelve hours, when the finger was again resorted to. Digital compression was this time kept up for eight hours (eighteen and a half hours in all), by which time pulsation had ceased, and the tumor was hard and firm. The patient was placed at rest, and all pressure taken off. The tumor remained firm, contracting somewhat, and about three weeks after his admission, the patient was discharged cured.

Remarks.—In looking over the foregoing cases, certain conclusions present themselves. First, as to the etiology. Syphilis was absent in every case, and probably plays a less important rôle in the causation of aneurism than is often claimed for it. In four of the cases (I., III., IV., V.), the origin of the aneurism could be traced distinctly to a local injury to the vessel, while in case VI., it was probably due to some strain or injury received while walking. In case II., the

history was so indefinite as to preclude even a conjecture as to the origin of the aneurism.

As to treatment, the results show that treatment by digital compression is infinitely safer than ligation of the vessel. The two cases (II., VI.) treated by compression recovered rapidly, without an adverse symptom or complication; while of those ligated, two (III., IV.) died and one (V.) recovered. And it does not seem necessary to keep up digital compression unremittingly, but it can be substituted, without much retarding the cure, by pressure made by a tourniquet. Compression, by means of Esmarch's bandage, proved futile in the one case (III.) in which it was tried.

MEDICAL PROGRESS.

TREATMENT OF GOITRE BY IODOFORM.—M. BOÉCHAT has employed iodoform in the treatment of goitre (*Correspondenz, Blatt für Schweizer, Aerzte I., S. 12*): 1. External applications; the author made use of a glycerole of iodoform, which he protects by a coating of collodion. His results were negative in old, cystic, or parenchymetous goitres; in recent goitres, of soft consistence, on the other hand, the tumors diminished more rapidly than with either iodine or iodide of potassium; the odor, however, is a great inconvenience.

2. Internal use of Boéchat prescribed iodoform in pill of ⅙ grain, not to exceed ten daily. This treatment was only made use of in two cases of goitre of long standing, and his results are not stated.

3. Interstitial injections. M. Boéchat has made use of this method in three cases. The first case had had a goitre from infancy; the half of a Pravaz syringeful of a saturated solution of iodoform in ether was injected daily for fifteen days, and although the tumor diminished considerably in size, the treatment had to be abandoned on account of the inflammation produced. In the second case, two injections were sufficient to produce improvement in a case of long standing. In the third case, one of long-standing suppuration was produced without any other change.—*Journ. de Méd. de Paris*, Dec. 24, 1881.

SYPHILIS OF THE HEART.—Dr. MAYER (*Prak. Arzt., No. 4*) states that the symptoms of syphilitic affection of the heart are like those of the weakened heart of Stokes, or of chronic diffuse myocarditis. He describes the case of a patient who exhibited the symptoms of cardiac insufficiency without evident valvular disease. He had had syphilis eight or nine years before, periostitis was present in the leg, and the mouth was also affected. Treatment by mercurial inunction and iodide of potassium effected a complete cure.—*Practitioner*, Jan., 1882.

ATROPIA IN MENORRHAGIA AND HÆMOPTYSIS.—Dr. TACKE (*Berliner Wochenschr.*, 1881, No. 6) considers atropia a much more certain remedy in menorrhagia and hæmoptysis than ergot. He was led to apply it in these conditions by injecting it subcutaneously in a woman suffering from eczema, and who had at the same time a profuse menstrual flow. He injected $\frac{1}{100}$ of a grain twice daily for two days. The eczema was greatly benefited, and the menstrual flow was much reduced. He recommends a solution of sulphate of atropia 1 to 1000 of water. Five minims to be injected two or three times a day subcutaneously.—*Practitioner*, Jan., 1882.

EXCISION OF THE TARSAL ARCH.—Excision of the tarsal arch has been now so often performed for obstinate cases of club-foot, that some material for forming a just appreciation of its value exists. The questions that arise are these: Are there cases of club-foot in which the usual treatment by manipulation fails? If so, is excision of the tarsal arch the best means of dealing with them? Can this operation be wisely substituted for the milder treatment in other simpler cases? As Mr. Hayward insisted at the Clinical Society, there are very few, if any, cases *in children* in which tenotomy and judicious and long-continued after-treatment will not accomplish cure; and we hope to see this operation reserved entirely for adults, in whom the deformity of the bones and other changes have rendered other methods of treatment useless. In patients of the poorer classes, however, there is another consideration to be weighed—that is, time. A successful excision of the arch effects a cure in a few months, while an obstinate case in a child, ten years of age, say, may require as many years of skilful treatment by other means; and when the effect of the treatment is such that a cripple becomes able to compete successfully with his fellows in almost all kinds of labor, the question of time is of serious moment. But in suitable cases it is open to doubt whether the results of this operation are superior to those of Chopart's amputation, which is its alternative, while it would seem that its dangers are greater. Mr. Davy has lost by death one out of seventeen operations, and König has lost one out of three, both patients dying from septicæmia. The danger, then, of excision of the tarsal arch ought effectually to prevent its being substituted for the more general treatment of club-foot in simple cases, even although it cures in a shorter time; and it certainly has not yet been demonstrated that its results are superior to those of amputation, which is safer. It is eminently a case in which a judicial investigation might be expected to do good.—*Lancet*, Dec. 24, 1881.

MALFORMATIONS IN THE REGION OF THE OMPHALO-MESENTERIC DUCT.—Prof. ROTH (Basel) concludes an interesting article (*Virchow's Archiv.*, Bd. 86,) on malformations in the region of the omphalo-mesenteric duct, with the following summary of those hitherto observed:

1. The common *diverticulum ilei* (Meckel) which usually hangs free in the abdomen, but may be found in a hernia or in the folds of the mesentery.

2. The adherent diverticulum; the union, by means of a blind end or a cord, is commonly at the navel, more rarely at other points in the abdomen.

3. The open diverticulum; the orifice being at the navel. It may be surrounded by a small prolapse of the wall, or a red, saccular prominence, or there may be prolapse of the bowel.

4. The diverticulum is the starting point of retention tumors-enterocysts, which may retain a connection with the bowel or be isolated.

AMMONIACAL PEPTONATE OF IRON.—To prepare this solution, MM. JAILLET and QUILLART recommend the following formula: Dried peptone, 5 grammes; sol. of perchloride of iron, 6 grammes; neutral glycerin, 50 grammes; cherry-laurel water, 150 grammes; water of ammonia, q. s. Dissolve the peptone in 50 c.c. of cherry-laurel water, and then add the glycerin; then dilute the officinal (French) solution of perchloride of iron with 50 c.c. of cherry-laurel water, and mix the two solutions; then add ammonia drop by drop until a flocculent precipitate is formed, which dissolves on a further addition of ammonia. It should, however, be avoided having the ammonia in excess. Finally, add enough cherry-laurel water to bring the mixture up to 200 c.c., which will therefore contain two mgrm. and a half of iron in each Pravatz syringeful.

In these conditions the peptonate of iron has not the styptic taste of the perchloride, and when injected

hypodermically at the body temperature produces neither pain nor inflammation. It is also interesting to notice that the peptonate of iron does not give the reactions of iron with the ordinary reagents.—*Bull. Gén. de Thérapeutique*, Dec. 30, 1881.

THE INDICATIONS FOR TREPHINING IN FRACTURES OF THE CRANIUM.—From an interesting review of this subject, with the study of several cases, M. G. FELIZET concludes:

1. That far from adding to the gravity of a wound of the head, with circumscribed depression and fracture of the internal table, antiseptic trepanning is a perfectly harmless procedure.

2. The operation can only be governed in its execution by rules which vary with the circumstances of the case; it has the advantage of transforming a complex and dangerous wound, and one liable to infection, into a simple, antiseptic wound, and one easily examined. —*Arch. Gén. de Méd.*, Jan., 1882.

PILOCARPINE IN DIPHTHERIA.—From the study of a number of cases, Dr. GREZA FALUDI believes the following conclusions to be warrantable:

1. Pilocarpine, administered internally, is a remedy of incontestable value in diphtheria.

2. Pilocarpine will always be useful in inflammatory affections of the mouth or pharynx, on account of its property of producing excessive salivation. Nevertheless, it does not appear to be superior to chlorate of potash or chloride of iron, and can by no means be regarded as a specific.

3. It is especially of service in light cases of diphtheria when the affection is limited to the buccal and pharyngeal cavities.

4. Unfortunately, it does not prevent the morbid process from invading the larynx and respiratory passages, and then a fatal termination is usually to be expected.

5. When this invasion of the air-passages occurs, then pilocarpine is of no value.

6. When administered to young children, its consecutive depressive effects must not be lost sight of; under such circumstances stimulants should be associated with it.—*Bull. Gén. de Thérapeutique*, Dec. 30, 1881.

LUMBAR COLOTOMY.—At the meeting of the Société de Chirurgie, held December 28, M. TILLAUX stated that he agreed entirely with the opinion of M. Trélat, expressed at a previous meeting, that although a lumbar artificial anus was much more difficult to establish than one in the iliac fossa, it was much less dangerous than the latter, as all danger of peritonitis was avoided, and besides it incommoded the patient less. He also presented a patient on whom he had operated three years previously according to this method, for stricture of the rectum of inflammatory origin. An interesting point in the case was that the patient always experienced a desire to defæcate before the escape of the contents of the bowel, and was able voluntarily to restrain for a time any fæcal evacuation.

M. TRÉLAT also presented a case on whom he had operated a month before for cancer of the rectum; the effects of the operation had now passed off, and the fatal result had undoubtedly been postponed.

M. DEPRÈS stated that he had never seen a case of cancer of the rectum in which this operation could be required, and he had usually no difficulty in passing the entire hand through strictures of the rectum which were inaccessible to the finger. When he finds a case suffering from symptoms of obstruction, he applies a blister to the abdomen, and places the patient in a warm bath, as he cannot comprehend the utility of this operation in patients condemned to die from cancer.

M. LABBÉ said that he had made use of this operation for more than three years, and was more than ever impressed with its value, especially when the artificial anus was established in the lumbar region. He was surprised at the opinion of M. Deprès, for, although without doubt the cancer continued to develop, symptoms of obstruction were removed, pain was relieved, and he believed that life under such circumstances was prolonged for from eighteen months to two years.—*Gaz. Méd. de Paris*, Dec. 31, 1881.

VESICANTS IN PLEURISY.—From a study of six cases, Dr. BONAMY thinks that blistering is too much neglected in the treatment of pleurisy, and he believes that both theory and clinical experience justify its use in the decline of the affection. He suggests that perhaps the increase in the mortality from pleurisy noticed in the last few decades may possibly be attributed to this neglect of a valuable remedy.—*Bull. Gén. de Thérapeutique*, Dec. 30, 1881.

TREATMENT OF PERTUSSIS.—Dr. FORCHHEIMER recommends, as a modification of the treatment suggested by Letzerich, the insufflation of quinia, two parts to one part each of bicarbonate of soda and powdered acacia, the powder being blown into the larynx by an ordinary laryngeal insufflator, three or four times daily. The first effect is to cause a new paroxysm of coughing, but it soon passes off, and the beneficial results are usually evident on the fourth day. He reports ninety-seven cases.—*Amer. Journ. of Obstetrics and Diseases of Women and Children*, Jan., 1882.

ON THE SELECTION OF SUITABLE MEDICINES FOR THE GENERAL TREATMENT OF SYPHILIS.—SIGMUND says (*Wiener Med. Wochenschrift*, Oct. 15, 1881), that the choice of a remedy in syphilis is to be determined on when the symptoms of the general disease are such as to afford sufficient indications for the means to be employed and the mode of its administration. Syphilis frequently runs a mild course and gets better spontaneously. Energetic treatment pursued too soon may be injurious, and is useless, while it is impossible by any treatment to obviate the general disease. At the same time, it is all-essential to devote the utmost care to the primary symptoms, to observe dietetic and hygienic rules, and to pay attention to any concomitant diseased conditions. Specific "antisyphilitic" measures are, in his view, to be first employed in the second stage of the development of syphilis, in the third to fourth month from its commencement.

Agreeably to our present knowledge, the choice must lie between preparations of mercury and iodine. These are now employed in a very different fashion from of old. The therapeutics of syphilis depend more than any other group of diseases on clinical observation, for experiments on animals have resulted, in its case, in little instruction in pathology and pharmacodynamics.

Mercury is to be regarded as the direct, iodine as the indirect, remedy for syphilis. As is well known, one can obtain the removal of its manifestations by the outward application of very simple preparations of the first, and simultaneously can favor the formation and growth of normal tissues. What we so frequently observe with regard to syphilitic products in structures patent to the eye goes on, without doubt, in those lying deeper, especially in the tracts of lymph and blood-vessels. The carriers of syphilis, "the white corpuscles," are destroyed and excreted, healthy red ones become more numerous, and nutrition in general improves. The clinician can so indubitably establish this with respect to the most varied forms of syphilitic products, that a different verdict can only be ascribed,

ssed, to a defective way of usi[illegible]e remedies; it being istinctly understood that [illegible]n the medicines all indica[illegible] aids to the removal of the disease are to be sedulous employed.

Iodine favors absorption both in and by means of the lymph vessels, and is thus the most powerful remedy in scrofulosis and in rheumatic and gouty dyscrasiæ. It eases the pain like a narcotic; it hinders inflammation and favors sleep; finally, it promotes the excretion of certain metals from the organism, as of mercury. Hence it is remarkably curative when used against the results of its misuse. Its internal exhibition does not permit the conclusion that the syphilitic virus is destroyed by iodine, but merely that the unburdening of the lymph and blood from the elements of the scrofulous, rheumatic, and gouty dyscrasiæ, as from mercury, makes more free the natural tendency of the organism towards recovery, and with this forwards its co-operation for the removal, or at least amelioration, of the syphilitic manifestations. Iodine exerts a plainly rapid influence on the gummata in cases where a complication, in general the scrofulous, exists. This, frequently long latent, is first evoked, even in those apparently robust, by syphilis.

The sedative influence of bromine on the nerve centres, particularly the cerebral, those of the heart, and sexual apparatus, makes it a useful auxiliary in the treatment of the syphilitic, although itself possessing no direct power over the dyscrasia.

Mild forms of syphilis disappear under the use of pilocarpine subcutaneously more rapidly than when treated by the expectant method, and severer improve. By its use alone, however, we do not obtain permanent cures, while its continued employment in increasing doses acts unfavorably on the health of the patient.

In the contest between mercurialists and anti-mercurialists which has been waged for four hundred years, were humanity and knowledge alone the motives in such a dispute, the public, the patient, and the medical man (too often difficultly placed as regards the latter) would all be gainers.—*Edin. Med. Journ.*, Jan., 1882.

ABSORPTION OF VIRUS FROM THE SURFACE OF WOUNDS.—At the meeting of the Académie des Sciences on December 22, 1881, M. DAVAINE read an interesting article on this important subject. Any exact knowledge as to the time in which cauterization must be practised to prevent absorption cannot but be of great value, as one may well hesitate to cause useless suffering by a tardy cauterization, or to expose the patient by a too prolonged expective treatment to the dangers of infection. The experiments of Renault on the poison of glanders, and those of Colin on anthrax, demonstrated that absorption from a subcutaneous punctured wound is extremely rapid, and that cauterization was useless when performed later than ten or twenty minutes after inoculation. M. Davaine, however, shows that certain characters of the wound may serve to delay the rapidity of absorption. Having placed material from a case of anthrax on the surface of wounds produced in rabbits by vesicants, friction with rough surfaces, or the removal of a small piece of skin, he found that many animals survived when the wound was cauterized with caustic potassa one, two, or even three hours afterwards. M. Davaine suggests as an anatomical explanation that in a sub-epidermic wound a number of small vessels are divided and the circulation is maintained by the collateral branches which are given off immediately below the divided vessel, and by this the poison, which has penetrated into the interior of a wounded vessel, reaches the general circulation. In a more extensive wound even the collateral branches will be largely destroyed and this direct entrance of the poison into the circulation will be retarded. Therefore, even though punctured wounds are the most dangerous, it seems prudent to cauterize infected wounds even if several hours have passed from the time that they were inflicted.

It may be noticed that the above results do not deal with the wounds inflicted by bites, although they apply closely to wounds such as a surgeon would be apt to inflict on himself with a knife. No allusion is made to the use of the hot iron, the cautery which would naturally suggest itself, M. Davaine having only made use of sulphuric acid and Vienna paste. It is to be hoped that this line of research will be extended.—*Gaz. Méd. de Paris*, Dec. 24, 1881.

CURE OF DEAF-MUTISM.—Two cases of deaf-mutism observed in young children, who recovered their hearing sufficiently for them to learn to talk, furnished M. BOUCHERON with the subject of a memoir, which he read at the meeting of the Société de Médecine de Paris, on October 8, 1881. The first case was in a child two and a half years old; the second, four and a half years old; both had strong constitutions and quick intelligence, never having had convulsions, and of healthy parents. In the first case M. Boucheron diagnosed pharyngeal catarrh, propagated by the Eustachian tube to the tympanum, the tube, later, being obliterated by the swelling of its mucous membrane. After one year the child was able to hear loud talking, and was able to pronounce very distinctly the words which it learned without paying any attention to the movements of the lips. The diagnosis was the same in the second case, who partially recovered hearing in a month, and after five months was able to pronounce the words which were heard. M. Boucheron compared the mechanism in the case of these deaf-mutes by compression of the auditory nerve to blindness produced by compression of the optic nerve in glaucoma, and proposed to designate the process by the term *otopiesis*. He thinks that this idea will suggest the line of treatment to be recommended, viz., the inflation of the tympanum by a catheter introduced during anæsthesia and the reduction of the pharyngeal inflammation by astringents.—*L'Union Méd.*, Dec. 25, 1881.

PALLIATIVE TREATMENT OF ATRESIA VAGINÆ.—Dr. A. F. ERICH reports a case of atresia vaginæ where an operation was positively refused. Menstrual molimina occurred at the age of fifteen. The abdomen began to enlarge, and she suffered intensely at every menstrual period, requiring large doses of morphia to control the pain. She had been under Dr. Erich's treatment for three years now, and requires no morphia at all. About a week before the beginning of the menstrual period she begins taking half-drachm doses of potassium bromide, three times a day. This reduces the hyperæmia, and so limits the amount of blood thrown out during menstruation. In the inter-menstrual period so much of the fluid portion of the blood is absorbed as to keep the tumor from enlarging, or slightly to decrease its size. She suffers at her menstrual periods no more than many women who consider themselves perfectly well. Dr. Erich suggests the further testing of this treatment in such cases.—*Atlanta Med. Register*, Nov., 1881.

LOSS OF SIGHT IN YELLOW FEVER.—In the *Archives of Ophthalmology*, for December, 1881, Dr. JUAN SANTOS FERNANDEZ reports three cases in which there was complete amaurosis in the course of yellow fever; in the only case in which an ophthalmoscopic examination was made, no abnormal condition was detected. He was also able to confirm the external changes known to occur in the cornea and conjunctiva, but the existence of amaurosis, which may be only temporary, has not been before reported.

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SATURDAY, FEBRUARY 4, 1882.

PREVENTION OF CONTAGIOUS DISEASES.

GRADUALLY but steadily the conviction has been gaining ground that diphtheria, like scarlet-fever and similar diseases, is communicable from person to person, either directly as, for example, in the notable case of a member of one of the royal families of Europe; or through the medium of the air, or of various substances which have come in contact with the products of the disease. Instances have been cited which seem to indicate that milk has sometimes been the vehicle of introducing the poison into the system.

The fact of the contagiousness of this disease, however well established it may appear to be to the medical profession, is not sufficiently well understood by the general public, especially in its relations to the employment of the proper means of prophylaxis. Hence, one of the chief causes of failure in the successful application of measures designed for modifying, checking, or preventing the progress of epidemics of infectious diseases, is the want of the intelligent co-operation of the public, springing mainly from ignorance of the nature of these affections, their mode of propagation, and particularly of the course to pursue to prevent their spread. Every medical officer of health will corroborate this statement by his experience in numberless cases where the grossest ignorance of the plainest sanitary preventive measures has well-nigh thwarted his best-directed efforts to protect the public health. The experience of every physician bears evidence in the same direction.

It must be admitted that without plainly instructing the people in relation to the preventive management of contagious diseases, and, as inseparably connected therewith, in their duties to themselves, their families, and to the public, satisfactory progress cannot be expected in the work of restricting and intercepting the propagation of these affections, which, though in a great measure preventable, are constantly making their presence seriously felt in every community. Sanitary laws and ordinances are eminently proper and absolutely necessary, but though well conceived in the first place, and well understood by the sanitary authorities, they fail or are only partially effective in their application, unless the people are made familiar with their meaning and objects, and are instructed by plain rules in their obligations and duties.

As a medium of imparting this information, physicians hold no secondary position, and in this relation sustain a very weighty responsibility to society. Their early information of the case—almost invariably in advance of that obtained by the health officer—affords an occasion and advantage of vital importance for promptly instituting decisive measures. Even when there exists only a suspicion as to the nature of the disease, which may scarcely justify a report to the proper authorities, the exercise of the utmost caution and foresight will not be unproductive of good, especially should the subsequent development of the case confirm this suspicion. Their opportunities for directing and controlling the proper protective measures are at all times specially prominent. Their relations to the family enable them to exert a more than ordinary influence in this sphere of action. Their co-operation with the health authorities is of the greatest value in their efforts to compel observance of the salutary provisions of the health ordinances. The physician cannot divest himself of this additional and weighty responsibility, and the more fully he realizes it, the better fitted will he be to discharge the duties of his high office.

However valuable and indispensable the services of the medical profession, either in the capacity of medical advisers or as private citizens, may be in the dissemination of useful information upon important sanitary topics, there is still great need of a more general, comprehensive, and systematic means of educating the masses of population in this special knowledge, particularly in respect to the requirements of preventive medicine. Plain, systematic, authoritative instruction in the details of the management of contagious cases, should be imparted to every responsible member of the community, and this should emanate from the board of health or sanitary council. There is no better or more suitable method of carrying out this aim than by the use of circulars, describing in plain and unmistakable language the chief features of domestic sanitation and

its relations to public health, and the precautions necessary to observe in all diseases dangerous to the community. These circulars should have the widest distribution, and should form the invariable guide when cases of disease occur. Several American local and State boards of health—notably that of Michigan—have adopted this plan, in several instances, with fruitful results; but the method is capable of more general application than it has yet received, and, happily for the future, this conclusion is beginning to be more fully realized.

These tracts would be specially valuable in the hands of the physician, as they could either take the place of or supplement his advice, and being authoritative would be more likely to command observance, especially after they had received his endorsement. Their convenience, where it may be difficult to communicate verbally all the details of management affecting the family and society understandingly, is at once apparent; so, likewise, in cases where there is intelligence and willingness, but where all the minutiæ have not been explained or comprehended, and therefore have failed to be carried into operation.

But the prominent object is to instruct the public—not simply those employed immediately about and associated with the sick—so that they shall at all times be prepared for any emergency, and be able to judge intelligently of the proper course to pursue. It is also to foster a healthy popular sentiment against the infringement of sanitary protective law, based upon a clear conception of its meaning and importance. Books of a class of which the Health Primers are a type are exerting a good influence in this same direction, by disseminating sound, practical information on subjects that so intimately concern the public. The American Public Health Association is another valuable agency for spreading knowledge upon important sanitary topics. Through its members, who are now located in every part of the country, an impetus could and should be given to domestic sanitation in its various branches. Journals devoted solely or in part to the subject of public health are also an efficient instrumentality for the diffusion of useful knowledge, and their prosperity is an evidence of the existence of a public want, and of an appreciation of the effort made to meet it. The capability and special fitness of the medical press for rendering a fruitful service in the sphere of public instruction is not lost sight of, and the appreciation of its high office and grand function of unfolding the laws and teaching the principles of hygiene—the prevention of diseases—first to the profession and through them to the multitudes, is evinced by the custom with every journal of repute of regularly devoting a considerable space to the presentation and discussion of this subject.

OUR NATIONAL MEDICAL MUSEUM AND LIBRARY AND THEIR DANGER.

THE Surgeon-General of the Army has sent to Congress, through the Secretary of War, a special estimate for $300,000 for the purchase of a site in Washington, and the erection thereon of a fire-proof building, to contain the Records, Library, and Museum of his Department. This estimate is accompanied by plans of the building proposed, which is recommended to be of brick, and to be a plain, substantial, thoroughly fire-proof building. The estimate and plans are now before the Committees on Public Buildings and Grounds, in the House and Senate respectively, and it is highly desirable that these committees shall be informed of the opinions of those competent to judge as to the value of these collections and the necessity for a new building in which to store and arrange them so as to secure their preservation and greatest utility.

As to the value of the collections there cannot be a shadow of doubt; they are of national importance, and their value to medical writers and teachers, to those interested in sanitary matters, to medico-legal experts, and to scientific men of all branches cannot be estimated in money.

The library is the largest, and in every way the most valuable, of any collection of its kind in the world. The Museum contains 22,000 specimens, and is unique in the completeness with which it illustrates military surgery and the diseases of armies.

The Records consist of over 16,000 bound volumes of manuscript hospital records with a large mass of papers, including the medical records of our late war. The propriety of a reasonable expenditure on the part of the Government to secure the permanent preservation of these treasures and to make them accessible to those who wish to consult them surely requires no argument. And although the medical profession is especially interested in this matter, every man who is interested in the progress of medicine and the skill of physicians (and who is not?) should be equally interested.

These records, books, and specimens, are now contained in a building which is not fire-proof—which is unsafe as to its mere stability—which is insufficient not only for their proper arrangement, but even for satisfactory storage. It is surrounded by houses and sheds which are very inflammable and which are in immediate contact with its wings. The danger from fire is really a very serious and urgent one.

The accommodations for the officers and clerks are so limited as to cause serious inconvenience, and it needs but half an hour's inspection to convince any one that relief should be given as soon as possible.

We give elsewhere a list of the members of the committees on Public Buildings and Grounds, both for the House and the Senate, and we strongly advise those of our readers who have the personal acquaintance of any of these gentlemen to write to them at once and tell them what is thought about this matter.

More than this, we advise those who know any Senator or Member of Congress to take an early occasion to express their views on this subject and urge that the necessary legislation be passed to permit of the commencement of work during the coming summer. In this matter delays are dangerous.

RESECTION OF THE LUNG.

IN the MEDICAL NEWS for January 7, 1882, we called attention to the improvement in the method of treatment of localized pulmonary cavities by injection and drainage. We now find that attempts to extend this principle of surgical interference have been carried to the greatest length, as is seen in the number of attempts which have recently been made in Germany and France to demonstrate the possibility of partial excision of the lung without a necessarily fatal result. Dr. BLOCK and Dr. SCHMID, in the *Deutsche Med. Wochen.* for November 19, 1881; Dr. GLUCK, in the same journal for December 3, 1881; and M. MARCUS, in the *Gazette Méd. de Paris*, December 3, 1881, each report several instances in which they succeeded in removing portions of the lung in dogs and rabbits, the animals recovering from the operation, and in one case living as long as a year without apparent inconvenience from the mutilation. Schmid, indeed, reports one case in which he first excised successfully a portion of the right lung, and then six weeks later of the left; and he states that he intends to remove an entire lobe from the same dog in order to determine whether adhesions will increase the difficulty of the operation. Dr. Gluck, after satisfying himself of the feasibility of partial excision, and of the fact that ligation of the root of one lung did not necessarily produce fatal œdema of the other, attempted the removal of an entire lung in six dogs and fourteen rabbits. Of these, all but two died of pleuropericarditis, with fibrinous exudation. Two rabbits, however, survived without having shown any febrile reaction, only a transitory dyspnœa and no pulmonary œdema; and three months after the operation they were apparently in perfect health.

Dr. Block, who also, after numerous attempts, succeeded in a partial excision of the lung, states that the portion of the lung remaining expands sufficiently to fill the space occupied previously by the part removed, and that in successful cases no defect in respiratory murmur can be distinguished anywhere over the chest. He believes that this operation is destined to be generally performed in the majority of phthisical cases, gangrene of the lung, etc., as by it the patients not only are freed from the diseased organ, but receive a new, healthy lung in its place.

While these experiments may show that surgical interference with pulmonary disease is not so far removed from feasibility as generally supposed, and that pulmonary herniæ may perhaps with safety be excised, we can hardly conceive of any one foolhardy enough to act on the suggestion of Dr. Block and attempt the excision of a tubercular pulmonary apex.

VACCINATION.

THERE is no graver responsibility resting upon the medical profession than that which is involved in the question of vaccination. After a long and successful reign, the belief is again being fully investigated, both by the public and the profession, is vaccination as fully protective against small-pox as it was once thought to be? The question admits of an easy and most decided answer—that there exists no particle of evidence that a recent properly performed vaccination is in any degree less of a safeguard against variola than it has always been claimed to be. If this dogmatic assertion is true, and it cannot successfully be assailed, it is incumbent upon us to account for the fact that cases of variola have been observed in the persons of those who have been recently vaccinated. But this fact will present no difficulty to those who have observed the carelessness with which the operation has been done, and the character of the virus used in many cases.

The occurrence of an almost infinitely minute proportion of cases in which syphilis has been said to be conveyed by vaccination, has led to a most unfounded prejudice against humanized virus, and has made its employment often impossible, although most excellent authorities, and some of them those possessing the largest experience, rely upon it to the exclusion of all other, as more regular, more rapid in its course, and equally efficient.

It is just here that the duty of the profession to the public is afforded a most important field to labor in. Where from prejudice, fashion, or any other reason it is necessary to resort to bovine virus, if the same caution is used in the selection of that virus as is bestowed upon that of a crust from the human arm, quite as efficient protection will attend the performance of the operation.

The pedigree of the virus, that of the animals from which it was procured, and upon which it is propagated, and the amount of dilution to which it is subjected before being placed in the market, are all points which should receive the closest attention. Indeed, our representative medical societies can do

no better work than to carefully examine and report upon the system pursued, and the painstaking care bestowed upon the business, in the numerous "vaccine farms" which so loudly claim the patronage and endorsement of the profession.

Upon individual members of the profession rests the responsibility of so carefully performing the operation, and so critically scanning its results, that the public will cease to regard it as a simple mechanical procedure which can be successfully practised by any one who becomes possessed of a "point." When the same conscientious care is bestowed upon vaccination, as was formerly done by our predecessors, we may expect to see the same efficiency in its results, and the public will be ready to recognize its value by a suitable honorarium.

When vaccination is carefully performed, and practitioners refuse to recognize any case as being successfully revaccinated until certain specific results are obtained, altogether independent of the question of soreness, we shall cease to hear of variola appearing after a recent vaccination.

One of the chief objections encountered by the advocates of compulsory vaccination, and, indeed, almost the only cause for opposition to it among educated people, is the possibility of the transmission of syphilis by its means. As animal lymph has been shown to have equal protective power with the humanized lymph, and as horned cattle are known, on the authority of MM. Depaul and Ricord, not to be liable to syphilis, this obstacle was largely removed, but now the question of the possible communication of other diseases from cattle to man is being raised. We are told, however, by Dr. Dompeling, the director of the Utrecht vaccination station, that veterinarians in Europe are of the opinion that the diseases of calves and heifers are, on the one hand, not transmissible, and on the other, have such distinctive characters as to be easily recognized and avoided. Dr. Creighton, of Cambridge, is, however, convinced from numerous experiments that it is possible to propagate tubercle from one species to another, and, although his views are combatted by such authorities as Drs. Günther, Thudicum, Carpenter, and others, he yet undoubtedly brings forward strong presumptive evidences of the truth of his statements. At Utrecht the *parc vaccinogène*, or "vaccine farm," is an annex of the veterinary school, and Mr. Ernest Hart, who opened the discussion on this subject at the late meeting of the British Medical Association, is of the opinion that to render precaution complete, it would be serviceable to have a veterinary surgeon attached to every such institution, thus ensuring the genuineness of the lymph, and its freedom from noxious qualities.

DARWINISM IN MEDICAL APPOINTMENTS.

THE doctrine of heredity is one of the most interesting offshoots of the Darwinian speculations, and is capable, when it shall be thoroughly investigated by accurate observers, of throwing much light on many obscure points in the social organization, as well as in the animal world. It is true that in its applications to dynastic succession the result has not always been happy, according to current historical beliefs. Still, this may be owing to the prejudices of partial or mendacious chroniclers, and further investigation may show the current notions to be erroneous, as in the case of Henry VIII., whom Mr. Froude has demonstrated to have been a model husband, or of Lucrezia Borgia, whom a learned German has proved to have been a pattern of all womanly virtues.

Our attention has recently been called to a new development of the truths of heredity, quite as convincing as the fact that the domesticated dog will always turn round once before lying down establishes his descent from the wild animal, which was obliged to take this precaution to insure that his rest would not be rudely disturbed. For the benefit of science, therefore, we are glad to be able to announce the generalization from the isolated observations of several very wise governing boards, that the qualities which render a man well fitted for the duties of a medical teacher or of a hospital physician, and acquire for him just reputation, are transmissible to his offspring. Even the experience which is requisite to the safety of the indigent sick, who seek the hospital bed as a last resort in the struggle for life, comes by inheritance, as well as the faculty for imparting instruction on which the student depends for his success in the profession, on which he has staked his career.

Any one who may entertain the old-fashioned notion that an appointment to a professorship or to a hospital position should be a recognition of professional eminence or of ascertained fitness should hasten to clear his mind of such conservatism; it may now be fairly conceded that the theory is established that an applicant's claim to so important a position must no longer be based on aptitude, long training, experience, and reputation, but on his ancestors, since talent is hereditary and special characteristics are developed by the devotion of several generations to the same pursuit. By a judicious system of intermarriage it is believed that these hereditary excellences may be still further intensified. Indeed, there is no telling to what extraordinary official merit this fact of heredity may lead, or how widely its application may extend, if only the vulgar prejudice in favor of proved capacity can be excluded permanently from the minds of trustees and hospital managers.

We trust that no uncharitable observations by disappointed aspirants will be permitted to obscure the brilliancy of this useful addition to our store of scientific observation. Perhaps not the least of the subsidiary benefits which it will confer will be to release the boards of all our institutions from the harassing canvasses to which they have heretofore been subjected. When we recall the perplexing care with which a conscientious manager or trustee would weigh the qualifications of the several candidates, in order to discharge the very important duty involved in his trust of selecting among numerous prominent and capable competitors the one best fitted to bear the tremendous responsibilities of the position, we recognize fully the relief which will hereafter be experienced when it is necessary only to ask if a man be the son of his father, without troublesome inquiries as to his age, color, or previous condition of servitude.

THE INFECTIOUS CAUSES OF INFLAMMATION.

WE ask attention with peculiar pleasure to the lectures of Cohnheim on the infective causes of inflammation, which appear in this, and the number of the NEWS for January 21. These lectures are furnished us by the courtesy of their author, and they will be embodied in the forthcoming new edition of his *Allgemeine Pathologie*. Our readers are, therefore, enabled to see these important observations in advance of their publication elsewhere. The existence of a *contagium vivum* is strongly supported. All infectious diseases originate from a parasitic organism. The observations of Koch on the life-history of the *bacillus anthracis*, threw a strong light on the nature of these bodies, and on their mode of reproduction. Prof. Cohnheim maintains the view that there is a special organism for each form of infective inflammation, and he traces the changes in the walls of the vessels to *schizomycetes*. But our readers will, we doubt not, read these lectures of the master, and hence we proceed no further in our analysis of them.

HIDING SMALL-POX.

THE recent action of the Philadelphia Board of Health in censuring a homœopathic practitioner of medicine for not duly reporting a case of small-pox, calls attention to the startling dangers that attend every case unless the utmost care is taken to prevent the spread of the disease. It is stated that in this case there was a funeral attended by forty or fifty persons and that neither the clergyman who conducted the services, nor the friends who were present were informed of the nature of the disease. None of these persons took, therefore, any precautions against it. The friends and servants who nursed and attended the patient, were kept in ignorance of the true nature of her disease. One of the latter, who had not been revaccinated, contracted the small-pox. Her case was promptly reported to the Board of Health by a regular physician who was called in to attend her, and the arrival of the ambulance to convey her to the small-pox hospital, where she subsequently died, was the first intimation that the boarders had of the existence of the disease in their midst.

Subsequently, when the true nature of the disease became known, at least one large school was closed because some of the teachers had been unknowingly exposed to the disease as boarders in the house in which the cases occurred.

It is not made the legal duty of the physician, under the rules of the Board of Health, to inform the occupants of a boarding-house of any case of small-pox which may break out in it nor to see that they are vaccinated, but surely, both from a prophylactic and a moral standpoint, it is his duty to do so, whatever may be the detriment to the mistress of the house or the inconvenience to the boarders.

We know also that not a few physicians neglect to report contagious diseases, other than small-pox, in spite of the positive legal obligations to do so, and we direct attention anew to the law for the purpose of urging obedience to its just requirements.

The addition to the later blanks in red ink at the foot requiring the physician to report whether he has forbidden the children of the family to attend school or other places of public resort is also a matter of prime importance, especially at this time.

It is high time also that, by law, funerals in all cases of contagious disease be made private, and published notice be given of the cause of death.

The reports just received from Mt. Carmel and Gratz, given in another column, afford additional evidence, if it be needed, of the necessity of stringent laws and their strict enforcement, for the prevention of the spread of contagious diseases.

REVIEWS.

DIE FUNKTION DER BOGENGÄNGE DES OHRLABYRINTHS. Von Dr. BENNO BAGINSKI. Berlin: Pamphlet, pp. 9.
THE FUNCTION OF THE SEMICIRCULAR CANALS. By Dr. BENNO BAGINSKI.

The obscurity associated with Menières disease from a pathological point of view renders any facts relating to the functions of the semicircular canals of the greatest interest. Among the numerous theories proposed to explain the mode of production of the peculiar movements which follow mutilation of these organs, there is probably none which has been demonstrated with any degree of certainty. The theory which has been most generally accepted is that disturbances in the pressure of the endolymph are accountable, and an experiment of Munk of increasing the tympanitic pressure, and consequently the labyrinthine pressure, by injecting fluid into the tympanum is often quoted in its support. Dr.

Baginski has, however, found that these effects are only produced when the pressure is increased sufficiently to rupture the membrane of the round window, by which means the fluid is permitted to penetrate to the brain through the communication of the aqueductus cochleæ with the scala tympani, and the latter with the sub-meningeal space. In all cases where the movements followed, it was found that inflammation or hæmorrhage was produced in the neighboring parts of the brain, and that the rapidity with which the symptoms appeared, and their severity, was proportionate to the irritating qualities of, and pressure exerted by, the injected fluid.

It will be recollected that irritation of the portion of the corpus restiforme, which lies immediately under the jugular fossa, has been found by Magendie, Schiff, and others, to cause analogous results, and Dr. Baginski points out that in both cases we probably have a similar mechanism, with the addition, in the case of the injections, of a labyrinthine irritation. To eliminate the value of the latter, Dr. Baginski employed the method of Heidenhain of removing a portion of the bulla ossea by which the canals are directly reached, and he found that dogs in whom portions of the bony labyrinth are removed became perfectly deaf, but showed no disturbances of equilibrium when the brain remained uninjured, even though the entire labyrinth underwent fatty degeneration. When, on the other hand, an entire semicircular canal was removed then the disturbance appeared, but could be directly traced to an inflammatory condition of the brain.

The author shows that a similar mechanism explains the losses of equilibrium in birds after injury of the canals, and denies there being any relationship between the location or direction of the canal injured and the character of the resultant motion. He concludes that whatever may be the function of the labyrinth, it is not for the purpose of serving "an equilibrium sense," and it is probable that the gross lesions of the semicircular canals in the rare cases of Menières disease, which have been carefully studied, have been associated with less evident lesions of the adjoining parts of the brain.

CORRESPONDENCE.

HYPERMETROPIA IN THE PUBLIC-SCHOOL CHILDREN.

To the Editor of the MEDICAL NEWS:

SIR: The correspondence columns of your last issue, page 113, contain a communication from Dr. Loring, of New York, criticising the report of Dr. Risley "On Weak Eyes in the Public Schools of Philadelphia." Taking hold of the loose statement as to "*all foreign statistics*," the writer endeavors to show that the percentage of hypermetropia found by foreign observers is in close accord with his own investigations, which he seems to think have been assailed; and he thus tries not only to convict Dr. Risley of great inaccuracy in his quotation, but also to throw doubt on the value of his entire work. By the sweeping "*all*" thus inadvisedly used, were evidently meant all which were in any way comparable in extent and thoroughness. As to this, Dr. Loring himself says as introductory to his own statistics (*Trans. International Medical Congress, Philadelphia*, 1876, p. 929): "I choose these" (Erismann's and Conrad's) "as examples, since they are the only ones which I know of where sufficient numbers were used, and where at the same time the conditions of investigation were the same, or nearly the same, in each. Thus they are all upon the eyes of school children between the ages of six and twenty-one years, and the trial-glasses and Snellen's type at twenty feet were the basis of examination."

The array of authorities which he cites would generally seem, at first glance, to bear him out most completely in his criticism; but an examination of the table in which Emmert summarizes these same results (and from which Dr. Loring seems to have taken his table), shows several important facts. Cohn, Erismann, Hoffmann, Gayat, Conrad, and Pflüger, are the only ones of the fifteen whose figures reach one thousand scholars. Of Cohn's work, Dr. Loring has said (*loc. cit.*) "but unfortunately Cohn's statistics, extensive as they are, are not suitable for our purpose; since from the manner in which they were compiled, no correct idea can be obtained from them of the frequency with which, in comparison to each other, the three different classes of refraction occur." Were this all as to Cohn's figures, they might pass; but while Dr. Loring quotes his two studies giving percentages of 2.3 per cent. and 7 per cent. respectively, he omits his examination *under atropia*, where the percentage of hypermetropia was *over 88 per cent.*—an omission no doubt accidental.

Gayat, among 1588 pupils, found hypermetropia *manifest* among only 6.8 per cent. Conrad, whose statistics Dr. Risley excepted through a mistake, found with the ophthalmoscope 70 per cent. of hypermetropes among the children of six and eight years of age, while his lowest figures were 23 per cent., and the average among his 1518 scholars must be above 50 per cent. (the full figures are not at hand). Yet Conrad's percentage is quoted by Dr. Loring at 11.76 per cent.—his showing in reality of the manifest, not the total hypermetropia, which Dr. Risley has evidently sought. Erismann and Pflüger, whose 6206 scholars make up more than half of the list Dr. Loring cites (Cohn omitted), give 43.3 per cent. and 44 per cent. respectively; while Emmert, whose figures are entirely ignored, although his summary of the work of others has apparently been employed, found among 2148 scholars 77 per cent. of hypermetropes.

The other investigators of Dr. Loring's list examined numbers of pupils varying from 122 to 800; yet the percentages thus obtained, and representing in almost all cases only the hypermetropia manifest revealed by the test of trial-glasses, Dr. Loring puts on the same footing as that derived from Erismann's 4300 pupils—a proceeding manifestly too unjust to need further comment.

Summing up the work of those whom Dr. Loring cites, excluding Cohn (and many others would be as rightly excluded for the reasons above given by Dr. Loring himself), and adding the work of Emmert, we find that 14,622 scholars in the various schools, gymnasia, and universities, were examined, and 5094, or 28.7 per cent. of them found to be hypermetropic—just double the figures given by Dr. Loring. Remembering the fact set forth by Dr. Risley and many others, that the test by trial-glasses and Snellen's type at twenty feet is quite inadequate to show more than a fraction of the total hypermetropia (Conrad found by the glasses only 16 per cent., and 70 per cent. by the ophthalmoscope, at eight years), and that only in a few of the examinations was this latter means employed at all—we have the fullest corroboration of Dr. Risley's results.

To cite a few examples: Cohn among 240 pupils *under atropia* found hypermetropia in more than 88 per cent.; Emmert among 1884 eyes of pupils between the ages of ten and thirteen, inclusive, found 80.6 per cent. hypermetropic. Erismann among the 59 pupils of his lowest grade found 67 per cent. hypermetropic and 55 per cent. among the 783 of his second; while Dr. Roosa, quoting his paper, says, "Erismann states that under atropia it is probable that there would be a larger percentage of hypermetropia than he found, and

that the hyperopic eye is probably the normal one in youth." (*Trans. Am. Ophthal. Soc.*, Vol. ii., p. 385). Compare these with Dr. Risley's figures and his 74 per cent. does not, at all events, stand alone; while a hasty study of the methods of examination employed in other published investigations, fails to show that anywhere the same thoroughness and precaution were used, Dr. Loring to the contrary notwithstanding. It may be that the proportion of hypermetropic children in the schools of New York City does not exceed an average of 13 per cent.; but I fear that any remark from an outsider as to the New Yorker's lack of far-sightedness would rouse an indignant rejoinder.

In the preparation of this hasty communication only a few of the publications involved have been attainable, and many quotations at second-hand have been therefore necessary; yet I believe it will throw a truer light on the subject than did the letter which called it forth.

Very respectfully yours,

B. ALEX. RANDALL, M.D.

1835 CHESTNUT ST., January 31, 1882.

NEWS ITEMS.

CINCINNATI.

(*From our Special Correspondent.*)

FREE VACCINATION at the Health Office has been suspended by the health board. Gratuitous vaccination, it was at first supposed, would be accepted only by the very needy of the city. But it was soon discovered that among the hundreds who daily applied were very many who were well able to pay for their attendance, as well as others who came from neighboring villages. The work has now been transferred to the district physicians in the different wards of the city, they being less likely to be imposed upon by non-residents and those in good pecuniary circumstances.

SPRING SESSIONS will be held this year, as usual, by the three regular medical colleges. These will begin about the middle of March and continue two months. They are designed principally as post-graduate sessions, and consist mainly of clinical, laboratory, and operative courses. These are not recognized by the colleges as regular lecture courses, such as are demanded of the candidate for the degree.

Summer graduating terms are here looked upon with disfavor, and are considered mere catch-penny affairs, and none of our colleges hold such sessions.

PITTSBURG.

(*From our Special Correspondent.*)

ARRESTS FOR ADVERTISING NOSTRUMS.—Through the efforts of Drs. Jos. N. Dickson, Wm. R. Hamilton, and one or two other physicians in this city, there has been, within the last ten days, several arrests and convictions of irregular practitioners and one druggist for "advertising medicines, drugs, and nostrums for the cure of secret diseases, etc."[1]

There are suits pending against Dr. Hartman and others who have escaped from the city and State. Dr. C. R. Lightner (a graduate of Bellevue Hospital Medical College) was arrested and convicted for advertising (in common with his partner, Dr. Hartman,) Pe-Ru-Na, which, it is stated, among many other things, will "restore sexual debility to the energy, fire, and vigor of youth." After conviction, Lightner was let off upon payment of all the costs and attorneys' fees, and his voluntary publication of a card in the daily papers, promising "not to distribute pamphlets recommending nostrums, etc., for private diseases and female complaints."

Another man was convicted for advertising "Ricord's Specific Pills" for gonorrhœa, gleet, and similar diseases. He is a druggist here, and an ex-president of the College of Pharmacy. He took high ground when arrested, and asserted he was being prosecuted for purposes of blackmail, but changed his tune when he discovered it was impossible to make any settlement of the case that would at all benefit the prosecutors in any way. To mend his ways and sin against public decency no more was the only ground upon which he could be let off. He, likewise, made solemn promises, and published a card, stating that until recently he was ignorant of the law touching advertisements of certain patent medicines and nostrums, and that since he has been informed of the law on the subject, he has withdrawn his advertisements, and he declares his intention to comply with the law.

Dr. George was arrested for a like offence, and lodged in jail in default of bail. He plead guilty, paid his fine on two charges, and promised to do better in future.

[1] *Vide* Pamphlet Laws of Penna., 16th March, 1870, forbidding publication of nostrums for secret diseases, etc.

SCOTLAND.

(*From our Special Correspondent.*)

ABERDEEN UNIVERSITY.—Sir Erasmus Wilson has just added to his munificent benefactions to science the gift of £10,000 for the institution and endowment of a chair of pathology in the University of Aberdeen. The professional staff of this medical school has long felt the want of pathological teaching, although, with the exception of Edinburgh, no other university in Scotland is provided with a chair for this subject.

UNIVERSITY OF EDINBURGH.—At present the degrees of Bachelor of Medicine (M.B.) and Master of Surgery (C.M.) are granted jointly or separately, after precisely the same examinations. Nearly all medical graduates have been accustomed to take both degrees, as two separate qualifications, one in medicine, and the other in surgery, are requisite for entrance to the army and navy, and for other public situations, although in point of fact the taking of the one implied the possession of the knowledge requisite for the other, and, if the extra fee were paid, the actual possession of the other degree. By recent powers obtained from the Privy Council, the Senate of the University has resolved for the future to grant the degrees always jointly, so that it will be no longer possible for any student to obtain the one degree without the other.

SMALL-POX.—The mortality returns for the week ending January 28, give the following number of deaths from small-pox. New York, 11; Philadelphia, 18; Boston, 6; Erie, Pa., 1; Richmond, Va., 21; Wilmington, Del., 1; Gould, Kan., 6.

The returns to the National Board of Health during the same period show 6 new cases at Dayton; 3 at Indianapolis; 2 at Winchester, Va.; 3 at Louisville; 16 at Gould, Kan. At Cincinnati 22 new cases were reported on the 28th, and 26 on the 30th. At Pittsburg 18, and at Allegheny 6 new cases were reported on January 31. During the month of January the mortality in Philadelphia, as compared with the same month of 1881, had diminished 60 per cent.

SMALL-POX IN MOUNT CARMEL, ILL.—Mount Carmel, Ill., has recently passed through an epidemic of small-pox which is interesting from the accuracy with which the starting-point of the epidemic was traced, and the marked character of the disease in the early cases. After exposure to the contagion of small-pox in St. Louis, where he roomed with a man who subsequently died in the pest-house of variola, a man aged

forty-eight years died suddenly in the cars while in the company of his daughter and some friends, and his death was not suspected until it was time to change cars. No eruption was found on his body with the exception of a discoloration said to resemble purpura, which existed on his body and thighs. No one was apparently aware of the fact that he had been exposed to small-pox. The funeral was largely attended. Fourteen persons were exposed to the disease in handling his body, of whom twelve had been vaccinated and two had not. Both these last died of small-pox. Of the former only one suffered, and he in a very light form, and recovered. Three others, unvaccinated and exposed in attending the first two, died of small-pox. Four others also had varioloid or small-pox and recovered. Three of them had been vaccinated; the fourth, who had not been, was an exceptionally strong and hearty man, to which, doubtless, he owes his escape. We are indebted to Dr. J. Schneck, Chairman of the Mt. Carmel Board of Health, for an admirable report of these cases, of which six died and five recovered. All of the ten cases could be traced to contact with the body in preparing it for burial, while ignorant of the cause of death.

SMALL-POX AT GRATZ, PA.—From a press dispatch of January 30, we learn that considerable excitement prevails at Gratz, a small town a few miles from Harrisburg, over the small-pox. Last week a prominent citizen died of small-pox, but the nature of the disease not being announced by the physician, there was an old-fashioned country funeral, at which hundreds of people were present. The sickness of three children followed, and of those at the funeral seventeen are now down with the disease.

HEALTH IN MICHIGAN.—For the week ending January 21, 1882, the reports indicate that tonsilitis, intermittent fever, membranous croup, and rheumatism have considerably increased, and scarlet fever, erysipelas, inflammation of the bowels, and cerebro-spinal meningitis have considerably decreased in area of prevalence.

Cases of small-pox were reported as follows: Twelve, at Leesburg, in Prairie Ronde Township, Kalamazoo Co.; six, at Geneva, Van Buren Co., (believed to have been brought from Chicago); seven (convalescent), at Bay City; two (new), at Grand Rapids; two, at Detroit; one (a tramp from Grand Rapids), at Marshall; one (new), at Monroe; one also in Menominee Co., and one at Escanabo, Delta Co.

NEW YORK STATE MEDICAL SOCIETY will hold its annual meeting at Albany, February 7, 8, and 9, under the Presidency of Dr. Abram Jacobi. Interesting papers have been promised by Drs. E. C. Seguin, E. R. Squibb, M. J. Roberts, W. C. Jarvis, Geo. H. Fox, D. B. St. John Roosa, E. Vanderpoel, L. Elsberg, D. Webster, L. D. Bulkley, T. G. Thomas, A. Flint, V. P. Gibney, S. Sexton, E. H. M. Sell, D. Lewis, of New York, W. W. Potter, of Buffalo, and Hailes, of Albany.

UNIVERSITY OF PENNSYLVANIA.—The ordinance conveying to the University a lot of ground in West Philadelphia was passed unanimously by Councils and approved by the Mayor on January 24. A preliminary injunction restraining the Mayor from signing the deed of transfer, was obtained by a local newspaper publisher, but on January 31, after a full argument, was dissolved by the Court. It is probable that the transfer will now be consummated, and that the University will enter into possession of a valuable addition to its landed property.

THE INTERNATIONAL MEDICAL CONGRESS OF 1884.—Acting under the powers delegated to them at the final meeting of the Congress, the Executive Committee of the late Congress addressed an invitation to Professors Hanover and Panum, Vice-presidents of the Congress, to arrange for holding the next International Congress, in 1884, in Copenhagen. This invitation has been promptly and cordially accepted, and the next International Congress will accordingly be held, in the year 1884, in Copenhagen.

MEDICAL SOCIETY OF THE COUNTY OF KINGS, N. Y.—At the annual meeting of this Society, held January 17, Dr. C. Jewett was elected President, Dr. Hopkins Vice-President, and Dr. Wyckoff Secretary.

QUARANTINE AGAINST PITTSBURG.—The State Board of Health of West Virginia has given notice of its establishment of quarantine against the city of Pittsburg, on account of the prevalence there of small-pox, and it has directed inspection to be made by its agents of all passengers, baggage, and freights coming into the State from that city.

A NEW PROFESSORSHIP IN THE PARIS FACULTY.—By a decree published on January 2, 1882, on the report of the Minister of Public Instruction, a Chair of Clinical Professor of Diseases of the Nervous System has been created in the Faculty of Medicine of Paris. By another decree of the same date, M. Charcot, Professor of Pathological Anatomy in the Faculty of Medicine of Paris has been transferred at his own request to this new Chair.

HARVARD UNIVERSITY.—A person desiring to give a fund for the medical education of women in the Harvard University, asked the President and Fellows whether such a fund would be accepted and used as designed. Majority and minority reports were submitted by the committee of the Board of Overseers in charge, and after a long discussion it was voted, eleven to six, to adopt the minority recommendation, which is to accept a fund, the income of which shall ultimately be used for the medical education of women.

MEDICAL PRACTITIONERS IN WEST VIRGINIA.—The State Board of Health has licensed eight hundred and forty-three practitioners in medicine and surgery in this State. Three licenses were issued during the past year to itinerant practitioners. It is stated that under the influence of the registration law quite a number of former practitioners are at present attending medical lectures to meet its requirements.

Among the diplomas offered for verification, were discovered a few of spurious character, which had either been directly bought, or obtained upon a nominal examination. These, of course, the Board rejected.

ANTISEPTIC METHODS.—In its review of the *Annus Medicus* the *Lancet* thus speaks of carbolic acid. Indeed, we may say that the day of carbolic acid is over, not so much because of its danger, as because we have found a substitute or substitutes for it as reliable but not so poisonous. Incalculable good, however, has been achieved by means of carbolic acid; it has been thoroughly efficient, as a rule harmless, easy of manipulation, cheap, and has rendered antiseptic surgery possible years before it otherwise would have been. Carbolic acid has not failed, but something better has been found. These substitutes are eucalyptol, iodoform, terebene, and resorcin. Mr. Lister uses eucalyptol gauze, and finds it quite reliable; iodoform has been used by him, but it is chiefly famous for the results obtained with it by Esmarch and his assistant Tilmanns. At present resorcin has not been much used in this country, but it promises well. Connected with this change is

the substitution of a dry-cotton dressing for the gauze used by Mr. Lister; Esmarch employs cotton impregnated with iodoform, and Gamgee has introduced pads of absorbent cotton rendered antiseptic by carbolic and salicylic acids. Both of these dressings appear to be fast gaining in popularity, and certainly possess some advantages over the gauze. The "spray" has been abandoned by many surgeons, and even Mr. Lister has spoken in qualified terms of its necessity; and had we to prophesy, instead of to record accomplished facts, we might venture to predict an early abandonment of this cumbrous addition to a surgeon's armamentarium. Catgut prepared with chromic acid and kangaroo tendons have been introduced as new forms of animal ligature. They both appear to be trustworthy; the chromic catgut is a great improvement upon the old catgut, and is, indeed, one of the most important improvements of Mr. Lister's treatment introduced in recent years.

COLLEGE OF PHYSICIANS AND SURGEONS OF CHICAGO.—We have received a preliminary announcement of this institution, which proposes to commence its first regular session about the 1st of October, 1882. It is stated that a pupilage of at least four years, including a graded course of instruction, covering at least three full winter sessions will be compulsory. Examinations for the degree of Doctor of Medicine will be conducted by a committee of examiners who have no other connection with the College.

The Faculty is not yet announced. Dr. A. Reeves Jackson is President, and D. A. K. Steele, M.D., Secretary of the Board of Directors.

HYGIENE IN SPAIN.—Spain is becoming more and more active in its participation in hygienic questions. A Hygienic Society has recently been formed, and approved by the King, of which the controlling section is located in Madrid with branches in all the capitals of the provinces and principal towns in the kingdom. Each section is composed of active and corresponding members, while the central society has the right of nominating honorary and foreign corresponding members. All who take an interest in hygienic science are eligible for membership. Each section is subdivided into sub-sections on general hygiene, epidemiology, climatology and medical topography, hygienic and medical statistics, and sanitary police.—*Gaz. Méd. de Paris*, Dec. 24, 1881.

MASSACHUSETTS GENERAL HOSPITAL.—The will of Mary Pratt, of Watertown, Massachusetts, recently deceased, gives $20,000 to this medical charity.

DR. BILLING'S ADDRESS ON MEDICIAL LITERATURE, read at the International Medical Congress, has been translated into French, and is being published in *L'Union Médicale* for January.

DIED.—At Edinburgh, on January 27, SIR ROBERT CHRISTISON, Bart., M.D., F.R.S. Sir Robert Christison was born on July 18, 1797, and was educated at the University of Edinburgh, where he received the degree of M.D., in 1819, and afterwards studied toxicology under Orfila in Paris. Soon after his return from the Continent, in 1822, he was appointed Professor of Medical Jurisprudence in the University of Edinburgh, and ten years later was promoted to the Chair of Materia Medica. In 1866 he received the degree of D.C.L. from Oxford, and LL.D. from his own University in 1872; he was created a Baronet in 1871. He twice held the position of President of the Royal College of Physicians, Edinburgh, and at the time of his death was Physician in Ordinary to the Queen for Scotland. His *Treatise on Poisons* is recognized as the standard work on the subject.

— At the end of December, at the age of eighty-four, M. BRIERRE DE BOISMONT, the well-known alienist. Born in Paris on the 18th of October, 1797, M. Brierre de Boismont received the degree of Doctor of Medicine in 1825. In the same year he published his *Elements of Botany*, and a treatise on *Pellagrous Insanity in Italy*. Among his other publications may be mentioned his work on *Suicidal Mania* and on *Hallucinations*.

NOTES AND QUERIES.

CONGENITAL BRONCHOCELE—VICARIOUS MENSTRUATION.

To the Editor of the MEDICAL NEWS:

SIR: In your issue of January 21, under the head of "Notes and Queries," I see a communication from Dr. M. R. Morden, Adrian, Michigan, asking if there had ever been any record of congenital bronchocele? I attended a lady on the 20th of this month, who gave birth to a male child apparently healthy, but I have since been called to see it on account of the anxiety of the parents about an enlargement of the neck, which I failed to notice (in my hurry) on the day of her confinement, and cannot, of course, be positive that it was present then; but it is certainly a veritable bronchocele. Will probably let you hear more of it as it progresses.

I would like now to ask if you or any correspondent can refer me to the record of any cases of vicarious menstruation?

Yours truly,
J. T. GORDON.

CARLYLE, ILL., January 25, 1882.

[Cases of vicarious menstruation are every now and then recorded in the journals. Our correspondent will find references to a number of examples in Churchill on the *Diseases of Women*. The latest recorded cases that we recall are one in the *New York Medical Journal* for August, 1881, p. 180, in which the patient vomited a considerable quantity of blood every month, and two in the *American Journal of Otology*, for April, 1881, p. 133, in which the hæmorrhage was from the ear.—ED.]

NATIONAL MEDICAL MUSEUM AND LIBRARY BUILDING.

THE plans and estimates for a National Medical Museum and Library Building, at Washington, have been referred by Congress to the Joint Committee on Public Buildings and Grounds, consisting of Senators Edward H. Rollins, New Hampshire; Justin S. Morrill, Vermont; Angus Cameron, Wisconsin; Charles W. Jones, Florida; George G. Vest, Missouri; and Representatives William S. Shallenberger, Pennsylvania; J. H. Lewis, Illinois; M. E. Cutts, Iowa; M. L. De Mothe, Indiana; J. A. Scranton, Pennsylvania; Nicholas Ford, Missouri; J. Hyatt Smith, New York; Philip Cook, Georgia; A. S. Hewitt, New York; J. W. Singleton, Illinois; H. A. Herbert, Alabama.

OFFICIAL LIST OF CHANGES OF STATIONS AND DUTIES OF OFFICERS OF THE MEDICAL DEPARTMENT, U. S. ARMY, FROM JANUARY 24, TO JANUARY 30, 1882.

STERNBERG, GEO. M., *Major and Surgeon.*—Granted leave of absence for one month, and during his absence Surgeon J. C. Bailey to attend the sick at Point San Jose, Cal.—*S. O. 9, Military Division of the Pacific and Department of California, January 16, 1882.*

COMEGYS, E. T., *Captain and Assistant Surgeon.*—Now stationed at Columbus Barracks, Ohio, to report in person to the President of the Medical Examining Board, in session in New York City, for examination for promotion, and, upon completion of examination, to return to proper station.—*S. O. 19, A. G. O., January 25, 1882.*

BANISTER, J. M., *First Lieutenant and Assistant Surgeon.*—Fort Reno, Indian Territory. Granted leave of absence for one month.—*S. O. 18, Department of the Missouri, January 24, 1882.*

THE MEDICAL NEWS *will be pleased to receive early intelligence of local events of general medical interest, or which it is desirable to bring to the notice of the profession.*

Local papers containing reports or news items should be marked.

Letters, whether written for publication or private information, must be authenticated by the names and addresses of their writers—of course not necessarily for publication.

All communications relating to the editorial department of the NEWS *should be addressed to No. 1004 Walnut Street, Philadelphia.*